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ERRATA.—The reader will please make the following corrections in Dr. Beatty's Dissertation on Congestive Bilious Fever, in the 2nd number of this volume—viz.

Page 157, second line from the bottom, for "vena cava hepatica" read "venæ cavæ hepaticæ," and wherever the same occurs in the essay.

Page 166, twenty-fourth line from the top, for "common" read "our main."

Page 168, sixteenth line from the top, for "and assisted again," read "or assisted occasionally."

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ORIGINAL COMMUNICATIONS.

ART. I.—*Thoughts on the Changes and their Causes, which are perpetually occurring in Material Creation.* By CHARLES CALDWELL M. D., Professor of the Institutes of Medicine, and Clinical Practice, in Transylvania University.

(READ IN THE LEXINGTON MEDICAL SOCIETY.)

MR. PRESIDENT AND GENTLEMEN,

There is reason to believe, that perpetual change is a fundamental requisite, and a never-failing phenomenon, in the condition of the UNIVERSE. It cannot be doubted, therefore, that it is the result of principles founded alike in wisdom and beneficence, and governed by laws established and administered by the AUTHOR of all things. Being, apparently, then, a favourite production of Divinity, it is worthy of the most profound contemplation of man. Hence I feel secure of your attention, while I make it the topic of a few observations.

As respects any changes of form that may occur in the entire mass of material creation, we might indulge in conjecture, but we have no knowledge. Nor, with our present limited means of instruction, is it probable that our information, with regard to them, will be rapidly accumulated.

But, as relates to certain subordinate portions of creation, the case is otherwise. On them our daily observation rests; of their condition and economy we are sufficiently informed; and there we know that ceaseless mutation is the established order. Of them, therefore, I feel myself authorized to speak; to dwell, for a moment, on the changes they exhibit, and briefly descant on the causes that produce them.

By astronomers the organization and government of the solar system have been long since explored, and satisfactorily ascertained. Nor have their inquiries been bounded by this portion of the heavenly bodies. Pushed far beyond such narrow limits, into the wide and gorgeous fields of space, so abundantly "fretted with golden fires," they have compassed in their ken other suns and other systems, magnificent in splendour, size, and arrangement, and almost countless in number. And, wide and varied as has been their range, wherever they have reached, perpetual motion and change are exhibited. While, in obedience to its own laws, each individual system changes incessantly its aspect and bearing, the whole, united in a confederate and universal system, and subjected to the influence of universal laws, move onward, at once, attracting and attracted, impelling and impelled, sustaining and sustained, in a march which constitutes the glory of creation.

This sublime and orderly progression of the heavenly bodies, constituting what is denominated the "harmony of the spheres" has furnished, for ages, matter of profound calculation to astronomers, a theme of enraptured song to poets and prophets, and a subject of intense admiration and delight to the entire world of cultivated intellect. Nor will it cease to sustain these varied relations, and produce these effects, while intelligence shall have being, and emotions of sublimity, grandeur, and beauty, shall belong to the soul. But to descend from this excursion through the heavens, to a sober survey of the globe we inhabit, respecting which our information is more minute, and our knowledge more accurate.

Here, mutations multiply, brighten, and magnify to the view, in proportion to our proximity to the scene of action.

The movement of the earth along the ecliptic path, gives to us, in succession, the variety, beauty, and indispensable boon of the changing seasons, while to its constant revolution on its own axis we are indebted for the grateful vicissitudes of day and night. To another of its movements are to be attributed, in part, the varieties we experience in relation to moonlight.

In the airy ocean by which our globe is surrounded, mutation is perpetual; for the most part beautiful, delightful, and salutary; but, at times, indescribable in grandeur and terror, and overwhelming in the temporary ruin it produces. It varies from the gentle fanning of the breeze, to the headlong and desolating sweep of the tempest; and from the fleecy rack, which flits across the sky, scarcely specking its cerulean brightness, or intercepting a sun-beam, to the deep gloom and appalling bursts and movements of the thunder-cloud. Nor must I forget the igneous meteors, of every description, at times most playfully brilliant and beautiful, and at other times sublime and threatening in aspect, which kindle their fires in upper air, the bow which suspends from the raindrops its celestial drapery, and the still more rich and radiant glories, with which the sun adorns the morning and evening clouds, as he emerges in the east, or tempers his fervors in the western wave. Such are a few of the atmospherical changes which are constantly presented to us; to which may be added, vicissitudes of temperature, whether sudden or gradual, with mists and rains, hoar-frost, hail and snow, and various others which time will not permit me to enumerate.

On the immediate surface, and to a moderate depth in the exterior covering of the earth, changes are perpetual, some of them slow, and innocent or useful, and others rapid, fierce, and destructive. Tempest-beaten cliffs are tumbling into gulfs or valleys at their bases, time-worn rocks are crumbling into sand and dust, and the very hills and mountains are gradually passing, with the torrents that wash them, into the narrow depths and humbler levels by which they are skirted. Nor is this all. Setting restraint at defiance, and outstripping time, in the march of their operations, the convulsions of the earthquake, and the

eruptions of the volcano effect, in an hour, mutations that might well be the product of ages. As if omnipotence were, for a time, consigned to their keeping, mountains sink and spring up, at their bidding, rivers forsake their long-worn channels, and fathomless lakes usurp the places where hills and plains, and other modifications of dry land had reposed.

Still more striking are the motions and changes which are incessantly manifested in the world of waters. The gushing fountain, the smooth-gliding streamlet, the dashing torrent, the deep and silent rolling of the majestic river, and the precipitate leap of the far-resounding cataract, which are the delight of the poet, and afford to the philosopher matter of contemplation, are subjects of observation, and well known to all.

But it is in the mighty deep, the "exhaustless source and repository of floods," that the most august and magnificent changes are exhibited. It is in the stupendous agitations of that immeasurable mass of waters, that the whole power ordinarily employed in the economy of the universe would seem, at times, to concentre its force.

Even in the depth of the most breathless calm, its heavings are not only incessant, but appallingly portentous, as indicative of strength which nothing can resist. They are the tossings of a giant, in a perturbed sleep, threatening to terminate in a paroxysm of rage. Its regular tides and currents constitute in it another kind of motion and change which never ceases. So do the disemboguing of rivers into it, and the ascent from it of its own waters, in the form of exhalation, by the influence of the atmosphere and the action of the sun.

But we find in winds the chief cause of the agitations it exhibits and the changes it experiences. These changes vary in extent and force from the curling of its surface, effected by the breeze, through the swelling waves, excited by the gale, to the foaming surf and mountain billows, the product of the tempest.

A further change in the world of waters, especially in the ocean, that claims our attention, is the result of the action of severe frost. This appears, in its maximum, in the polar regions, in the formation of glacial islands, of fearful extent, and of icebergs,

whose summits form a resting place for clouds. No wonder, then, that, in contemplating it, in its own vastness and power, and under the grandeur of these diversified and mighty changes, the high gifted poet should have hailed the ocean, in the following rich and magnificent apostrophe:

"Thou glorious mirror, where the Almighty's form
"Glasses itself in tempests; in all time,
"Calm or convulsed—in breeze, or gale, or storm,
"Icing the pole, or in the torrid clime
"Dark-heaving—boundless, endless, and sublime—
"The image of eternity—the throne
"Of the Invisible; even from out thy slime
"The monsters of the deep are made; each zone
"Obeys thee; thou goest forth, dread, fathomless, alone."

Another source of change in our globe, alike abundant and important, is that of propulsion, arising from the forcible impulse of one substance on another. Illustrations of this we find in the rebounding of bodies that have been hurried into collision, in the movement of those that have been subjected to a blow, and in the flight of arrows, bullets, and other missiles, directed against an enemy, or intended for any other purpose of destruction or amusement.

Of these various changes in material creation, all are not the product of the same causes; all do not result from the same principles of action; nor are they all governed by the same laws.

When matter moves in masses, and exhibits changes on an extensive scale, it is influenced by one or more of the principles of the attraction of gravitation, elasticity, or mechanical propulsion. Of this description are the magnificent movements of the heavenly bodies, the rush of the typhoon, and the whirl of the tornado, the bolt from the thunder-cloud, the convulsions of earthquakes, the eruptions of volcanos, the flight of arrows, bullets, and other missiles, the descent of falling bodies, the lapse of running waters, the phenomena of tides, and the raging of the ocean when lashed by the tempest. To the same principles are we to attribute the play of the mercury in the barometer, the elevation of water by suction and forcing pumps, the

action of the steam-engine, and the ascent of balloons, and other bodies inferior in specific gravity to atmospherical air.*

All these changes I say, with many other analogous ones, which the limitedness of my time forbids me to recite, are the product of principles denominated mechanical, which operate on matter in large masses. Nor are any other principles but these concerned *immediately* in the production of the movements which I have here attributed to them. Indeed it will be presently made to appear that there are three distinct sets of principles, with their kindred laws, which produce and govern the various changes that occur in all kinds of matter, of which we have any knowledge; and that they are so completely insulated, as to operate, each exclusively in its own sphere, and never to intermingle their influence or action.

These principles and laws, as may be stated now, with sufficient propriety, are denominated mechanical, chemical, and vital. Many changes in matter, resulting from the first set, having been already mentioned, I shall proceed to a recital of some of those which are to be regarded as the product of the other two.

Of the changes already referred to, a few are the offspring of chemical agency. These are the conversion of water into ice, vapour and steam, and the formation of gases in earthquakes, volcanos, and the discharges of fire-arms, and of gases generally, whether by artificial means, or in what is not inaptly termed the laboratory of nature. But as soon as gas and steam are generated, escaping immediately from the influence of chemistry, they obey, in their action, which is simple propulsion, the principles and laws denominated mechanical.

To the department of chemistry would seem to belong the changes of temperature characteristic of the different seasons of the year, and of day and night, as well as those sudden and

*I am not to be here understood, as asserting, that mechanical causes produce a stream of lightning, the imprisoned gas which engenders an earthquake, the steam in the boiler of the steam-engine, or the gas by which the æronaut inflates his balloon. My meaning is, that those agents, when in existence and operation, produce their effects mechanically.

sometimes great vicissitudes, from heat to cold, and the reverse, which occur irregularly in all climates, but most frequently and strikingly within the temperate zones, and in the vicinity of mountains, lakes, and arms of the sea. To the same department is usually referred, the formation of dew, the passage of vapour into clouds, its reconversion into water in the form of rain, its change by chrystallization, in the higher and lower regions of the atmosphere, into snow and hoar-frost, and the congelation of water, in upper air, into the meteor hail. In some instances even the inscrutable substances of light, electricity and galvanism, are, at once, subjects and agents under the influence of chemistry. Caloric, which, by its subtlety and power, approaches to spirituality, is always employed by it as its master operator.

But the scope of this rich and beautiful science is not confined to the atmosphere, and the surface of the waters and the ground. It penetrates the earth, makes its way to its most secret caverns and precious cabinets, where the diamond and the ruby display their lustre, and, besides many other manifestations of itself, is seen in splendour in stalactitic columns, and frost-work, the apparent production of the genii of the place, and, in all its glory, in the gorgeous furniture of the sparry cave. Nor do we fail to recognize its opulence in the abundant boon of the precious metals which it offers to our acceptance. Indeed the entire range of mineralogy, although not strictly and technically a branch of chemistry, is so far under its influence, that most, if not all the substances it embraces, received originally, from chemical principles, their texture and figure. Nor, in all these various productions and changes, have the mechanical principles had the slightest agency. By those of chemistry alone have they been effected.

Were I to descend to the curious but diminutive laboratory erected by man, and enumerate the various changes in matter, which are there produced, it would be little else than a rehearsal of what I have already represented, as done in the more magnificent workshop of nature. True, certain artificial processes, altogether new, are there instituted, and certain com-

pounds or simples, not hitherto found in nature's storehouse, are occasionally the result. Greater purity, moreover, is given to substances which nature presents to us. But still the principles which actuate, and the laws which direct, in every process, are those of chemistry; and may be included under the terms attraction and repulsion. They are the principles and laws which produce and govern the mutations of matter, not aggregated in masses remote from each other, but divided into its elementary parts in close approximation. I need scarcely add, that those principles and laws effect changes in such matter only as is destitute of life. On this point it shall be my business to dwell more particularly in a subsequent part of this paper.

But by far the most varied and curious, beautiful and interesting changes in matter remain to be mentioned; changes characterized alike by wisdom, goodness and power, and worthy, in all respects, of the creator, director, and sustainer of a universe. They are those that are manifested by the multitude of living organized beings, which constitute the vegetable and animal kingdoms. Of the immensity in number of these mutations, some conception may be formed from the fact, that they are as numerous as all the various phenomena exhibited by the untold myriads of individual living beings, vegetable and animal, that people our globe. For, extravagant as to some perhaps the assertion may appear, there are no two living beings precisely alike. Each one bears the mark of some change or modification of matter peculiar to itself.

The sources and grounds of the changes that occur, in living matter, are numerous. They begin with birth, are manifested in growth and decay, and consummated in death. After death, the body becomes again the subject of the principles and laws of chemistry.

The great instruments or rather means of the changes effected in the systems of living beings, are digestion and nutrition, secretion and excretion, absorption and intellection. On the operation of each of these let me beg permission to offer to your consideration a few remarks.

Let me previously, however, ask, why is it that the departments of chemistry and mechanics are considered completely insulated, and distinct from each other? I answer, because, in their operations, they never interlock; and because the principles and laws of the one can never produce the changes, nor exhibit the phenomena, that belong to the other. Hence, no one has ever alleged, or even suspected, that, in any process, they operate in common. On the contrary, they, in most, if not in all cases, oppose each other. The application of this will appear hereafter.

The changes which are effected in digestion and nutrition are striking and specific; such as never do or can occur elsewhere, because the processes themselves are specific, having no affinity or resemblance to any other. And from specific causes specific effects must necessarily issue. The powers which these processes possess and manifest, might be almost, without a figure, denominated *creative*. Plastic and convertive they certainly are, in a very eminent degree. Compound substances of one kind they convert into compounds of another, so totally different as to retain no similitude to those out of which they were formed. As relates to them, metamorphosis is no fable. In our own race, they convert common masses of dead alimentary matter into the symmetry and beauty, and endow it with all the energies, of the human system. The most exquisitely beautiful and lovely of our race are but manufactures from the viands that cover our tables. What would seem most extraordinary is, that it matters but little of what description or character those viands are. Whether they are fish, flesh, or fowl, animal, vegetable, or mineral, simple or multiplex, dressed plainly or in the highest style of art, the result is the same. They are converted into the curious being called man. Undergoing solution, peculiar in its kind, they are changed first into a white, and then into a crimson fluid, and, by reorganization, appear again in a solid form, characterized by the elegances and excellencies of the human frame. Were it not that familiarity takes from this metamorphosis its force and impressiveness, it might be deemed miraculous. To spread on a festive board a

course of common alimentary fare, and seat, at the head of it, a living Venus de Medici, in the vivacity of youth, the bloom of health, and the pride of beauty, and assert that the former could be converted into the latter, would be pronounced a fiction, of the wildest character, had not the experience of all time established its truth. And yet, as will appear, hereafter, some pseudo-philosophers have the folly and hardihood to pronounce this chemistry. As well might they contend, in their frenzy of hypothesis, that all the sublime intellectual operations of a Newton, a Leibnitz and a Milton were nothing but the ebullitions of an acid and an alkali. The very conception is treason against science and nature.

If we direct our observation to the lower orders of animals, even to those of the insect tribes, we shall find the changes effected in them by digestion and nutrition no less striking. Perhaps they may seem in some cases more so.

In many of those animals that subsist wholly on liquids there is much solid matter; and no small amount of fluids in those whose food is solid and dry.

The humming bird and at least one species of the bird of paradise sip, as their aliment, the saccharine but liquid and colourless secretion of the nectary of flowers. Yet do they possess bone, and muscle, and gland, and skin, and plumage of the most beautiful texture, and as gorgeous in colouring as the bow in the heavens.

Some butterflies are believed to banquet only on the most highly rectified and purest dew. Yet have they, in their bodies, no small amount of solid matter; while, in the exquisiteness of texture, and the richness and variegated tints and lustre of their wings, they set at defiance the language of description, and surpass even the highest execution of the pencil.

The common moth subsists on dry hair and wool, and the anobium maculatum, on the most perfectly seasoned wood; yet has each, in its system, a considerable amount of liquid matter. The same is true of the terebellum marmoreum, and the terebellum saxosum, although, as its name imports, the former subsists exclusively on marble, and the latter on stone of a differ-

ent character. Nor do the marble and stone resemble any more the animal fluids and solids, into which they are thus converted, than they do solids and fluids of any other kind. So wonderful is the metamorphosis, in animal nature, effected by the processes of digestion and nutrition. And yet, I repeat, that some men have the presumption and folly to denominate it chemistry.

Nor, in the vegetable kingdom, are the changes produced, by digestion and nutrition, less striking and beautiful.

Some plants feed exclusively on water, many depend for much of their food on the atmosphere, and not a few derive from that source their entire subsistence. Other plants extract, by roots, from the soil, the chief part, if not the whole of the materials that nourish them. Yet are these several families of plants equally healthy, vivacious, and flourishing, and, when subjected to analysis, are found to consist of the same elementary ingredients. This is no less true of plants that grow in different soils, and different atmospheres, remote from each other, than of those that grow in clusters in the same atmosphere, and the same soil. The elements that compose them are the same, and exist in the same relative proportions. Nor do vegetables exhibit, in their composition, any more of the elementary ingredients of their native soil, than they do of soils incapable of producing them. So entire is the change they effect on what nourishes them.

It is now, perhaps, the universal opinion of the most enlightened phytologists, and appears to be founded in truth, that many plants which seek stability, by rooting themselves in the ground, derive from thence no particle of nutriment. This is the case with some of the palms and canes, and not a few others of the most succulent plants of tropical climates. It is, in a particular manner, true of a great variety of those productions of the vegetable kingdom, which embellish, by the elegances of their form, and the splendours of their colour, the arid plains of the Cape of Good Hope. In that parched region of the globe, no rain falls, except during a few weeks in the winter, when active vegetation does not exist. During the summer and only produc-

tive months, so adust is the soil, that no portion of either moisture or vegetable nutriment can be extracted from it. Yet is the vegetable kingdom luxuriant and flourishing, almost beyond example. On the atmosphere alone, then, and the dews that fall, must the plants depend for all their nourishment. Heavy falls of rain, which have been known, on rare occasions to occur, prove highly injurious to them. The Solandra grandiflora, a beautiful flowering shrub of Jamaica, derives its nourishment solely from the atmosphere, in a parched soil, and is injured by adscititious supplies of water.

Many species of the genus cactus, of which the common prickly pear is one, depend for nutriment, exclusively on the atmosphere. Although a sandy barren soil affords them fixture, it gives them no food. They live on air and the moisture it contains. Hence, when completely deracinated, and suspended by cords, they continue to grow and flourish with vigour. And they yield, on analysis, the same component parts, whether they are connected with the earth, or hanging in air.

But perhaps the most remarkable instance of atmospherical subsistence, that can be placed on record, occurs in the case of the *epidendrum ærium*, an oriental plant, of great beauty, and exquisite fragrance. It is a native of Java, and India beyond the Ganges. At once to adorn and perfume their apartments, it is the practice of the inhabitants to pluck it up by the roots, and suspend it from their ceilings, by silken cords, richly ornamented by tufts and tassels. In this condition it is never even moistened by water, but subsists exclusively on the atmosphere around it. Yet does it continue to flourish in health and vigour, for many years, putting forth periodically fresh leaves and flowers, and pouring out copious streams of the most delightful fragrance.

Perhaps more than half of the very extensive family of the fuci have no roots, either to attach them to the soil, or to derive from it nourishment. They must necessarily, therefore, subsist on the atmosphere and moisture. Yet do they yield, on analysis, many elementary ingredients, which chemistry is unable to extract from the medium in which they live, and from

which alone they derive their aliment. Although the very lowest and simplest forms, then of living matter, still are they superior in their powers to chemistry.

One of the most luxuriant and celebrated plants that the earth produces, is known to subsist principally on water, and to form out of it all the elements it contains. It is the lotus of the Nile. So rare in beauty, and so delicious in fragrance, that, in the language of poetry, its very name is identified with voluptuous enjoyment, and so choice in all things, that it has been presented as an acceptable offering to the gods, that plant derives its nourishment, and forms, by its mutative energies, all its attributes, chiefly from the turbid waters of its parent river. A change more striking is scarcely to be found in material creation.

In the kingdom of living matter, secretion is another process abundant and diversified in the curious mutations and conversions it produces. The substance in which those changes are effected, is the blood of animals, and the sap-juice of vegetables; in each case the source of nourishment to the beings possessing it.

As the blood of animals is a fluid very strikingly different from the alimentary articles out of which it is formed, so are the secreted fluids, derived from it, no less different from the fountain that yields them.

They are, in the higher orders of animals, the gastric and pancreatic juices, bile, mucus, saliva, tears, oil, fat, semen masculinum, cerumen, perspirable and menstrual matter, the synovial fluid, the humours of the eye, the black pigment which imparts colour to the skin of the African, and the nervous fluid, supposing it to have an existence. To these may be added musk, castor, the offensive fluid emitted by the polecat, and a few other odorous matters which need not be recited.

Descending to the inferior orders of animal existence, we find there, the inky fluid of the cuttlefish, the electricity of the torpedo and the gymnotus electricus, the various poisons of the serpent tribe, and those of the scorpion, the tarantula, the asp, and other kinds of venomous insects and reptiles. All these are secreted matters, the result of changes produced by vital

action on the blood. A fluid *sui generis*, perfectly dissimilar to all others, is thus, by secretory economy, converted into numerous other fluids, neither possessing between themselves the slightest mutual family likeness, nor any likeness to their parent source. In each of these secreted liquors are ingredients found, which no analysis can detect in the blood.

Out of the blood, moreover, is prepared, by what may be correctly denominated a secretory process, the matter of bone, cartilage, tendon, muscle, nerve, brain, skin, hair, nail, and every other solid substance of which the body consists. This is another very striking instance of the multifarious product of vital action from a single fluid.

From the blood or sap-juice of vegetables, which, like the blood of animals, is a specific fluid, is also generated, by secretory action, a great variety of compound substances, equally different from each other, and from their parental source.

These are gums, resins, balsams, and oils, both bland and essential, camphor, asafœtida, and gum elastic, each of them a substance peculiar in character, all matters of colour and odour, as the green of leaves, and the innumerable hues and fragrance of flowers, bitter, acid, acrid, and saccharine juices, and the entire catalogue of vegetable poisons. Among poisonous juices may be mentioned those of the poppy, the tobacco plant, the datura stramonium, the nightshade, the henbane, the hellebore, the Kalmias, the rhuses, the mancinella, the upas, and many others, some of them natives of our own, and some of foreign countries.

All these are secreted substances, formed, by vital action, from the sap-juice of vegetables, and as different from *it* as they are from each other. As far as its character is known, the real blood or nutritious liquor of every plant, like the blood of every animal, is bland and innocuous; and exhibits none of the peculiar, and deleterious qualities, which belong to many of the substances formed from it by secretion. The real sap-juice of the poppy has in it no opium, that of the sugar-cane and sugar-maple, no saccharine matter, nor has that of the mimosa nilotica any gum arabic. The same is true of our muskmelon and watermelon vines. In the latter there is nothing sweet, and in

the former nothing fragrant. It is secretion alone, that gives those delicious qualities to the fruit which they produce.

A more complete and delightful metamorphosis the poet has never *imagined*, in his moments of inspiration, than that which the lotus, the jessamine, the Magnolia, and the rose actually *effect*, when they take up matter, which, far from being attractive to either of the senses, is indifferent or offensive, and convert it, by their peculiar action, into the surpassing beauty and fragrance of their blossoms. I repeat what I have heretofore uttered, that did not observation assure us of the reality of this change, we would regard it as a fable. The conversion of Daphne into laurel, or Progne into a swallow, would seem scarcely less incredible. Before dismissing the consideration of the changes produced in living matter, by digestion, nutrition, and secretion, a few thoughts on their causes will not, I flatter myself, be unacceptable to you.

As relates to the causes of the changes which occur in living matter, or the principles and laws by which those changes are produced and governed, three distinct hypotheses are maintained. These are the chemical, the vital, and the chemico-vital.

The first alleges that, both in its origin, changes, and dissolution, living matter is completely under the influence and governance of chemical principles; and that, therefore, in reality, every living body is nothing more than a chemical laboratory.

The second hypothesis places living matter, with all the changes and phenomena it exhibits, under the exclusive control of principles denominated vital. Those principles and laws it pronounces to be insulated, and, in all respects, as essentially different, and as radically distinct, from the principles and laws of chemistry, as they are from those denominated mechanical.

The third hypothesis professes to be a compromise between the other two, and, like most midway courses, is time-serving and unsteady. Its advocates are aptly denominated *chemico-physiologists*. This term is appropriate, and denotes very happily the character of those to whom it is applied.

These dogmatical expounders of nature become the victims of their own policy. By aiming at too much, they forfeit the

standing in science, which they might otherwise attain. Their grasping ambition impels them to make an effort to render the domain of chemistry coextensive with the limits of matter.

In their estimate of things, that science is necessarily concerned, in every change that occurs among the elementary particles of compound bodies.

Convinced, however, not only of the impossibility of explaining, but of the absurdity of attempting to explain, by means of chemistry, all the changes and phenomena that living matter exhibits, and, at the same time, determined not to abandon their unfounded claim on that elevated and beautiful department of nature, the "chemico-physiologists" have endeavoured to make that incongruous mixture of *immiscible agents*, from which they have so fitly derived their appellation. "True, say they, the various processes of living matter, such as digestion, assimilation, nutrition, and secretion, are not performed exclusively by chemical agency. They are functions partly chemical and partly vital. The changes they exhibit, therefore, in elementary matter, are the result of a co-operation of different principles".

Let us examine, for a moment, these propositions. Let us analyze them, and weigh them fairly in the balance of reason and sound philosophy, and see whether they are sterling in quality, and competent in weight; or whether, on the contrary, they are not counterfeit, in the one, and wanting in the other.

The principles of chemistry and vitality are universally acknowledged to be different from each other. Many who have studied them very zealously, and, perhaps, I might say profoundly, consider them as opposites. Facts are not wanting, which seem to indicate that they are so.

They are engaged, according to the hypothesis we are examining, in the performance of the same animal function. But they cannot be engaged on equal terms, else would they greatly interrupt, if not completely neutralize each other. They would exhibit the distraction, and destructive ravages of civil war. They would be a house divided against itself, of which common

sense and all experience unite with the oracles of revealed truth, in positively proclaiming, "that it cannot stand."

One set of principles, then, must be obedient to the other, and must submit to its control. It must become a mere instrument in its hand, to be wielded by it at pleasure. Its actions must be so changed and modified by it, as to be no longer its own; its influence swallowed up and lost in that of its superior.

The elementary particles of living matter cannot obey, at once, the laws of chemistry and the laws of vitality. They must "choose which of the two they will serve;" and, in doing this, they must "cleave to the one, and reject the other." This is no paradox, or tissue of sophistry. It is sober common sense. It is a primitive truth. A common and divided influence is a neutralized influence; or, rather, it is no influence at all. Nothing decisive can ever be effected by it.

Do chemical principles predominate, in the functions of living matter? The "chemico-physiologists" will not answer affirmatively. Truth compels even *them*, to reply in the negative, otherwise the functions would not be designated by the term *living*. Then must vital principles and laws predominate, and chemistry becomes, in these functions, a nullity and a name. It becomes precisely what vitality makes it. But vitality metamorphoses entirely its workings and results; and hence it is chemistry no longer.

We confess, say again the "chemico-physiologists" that digestion, nutrition, and secretion are processes, in which *ordinary* chemistry mingles not its influence. They are the result of *animal* and *vegetable* chemistry.

Is it, then, come to this! Have these self constituted philosophical potentates, who aim to be lords of material creation, and to subject the whole to chemical supremacy, fallen thus ingloriously from their "high estate" to take shelter under the subterfuge of a mere name? Do they imagine that a word can *be made* to alter the nature of that which it designates? that they can change the process by changing its appellation? The trick is too shallow, and cannot avail them. It is a feeble and disinge-

nuous bartering of substance for shadow; an abandonment of sense for a substitution of sound.

By this "precious confession" they have conceded every thing, and are a body militant no longer. They have acknowledged that the processes of digestion, assimilation, nutrition, and secretion cannot be performed by ordinary chemistry; and that is all for which the vitalists contend. By animal and vegetable chemistry can be meant nothing else than a system of operations and functions governed by animal and vegetable laws; and that is *vitalism*. It is a positive acknowledgment that, in living organized matter, the changes which occur are the offspring of principles and laws, insulated as to extent, specific in character, and as distinct from chemistry as they are from mechanics.

Do the chemico-physiologists contend, that although the processes of what they call animal and vegetable chemistry differ from those of ordinary chemistry, they are, notwithstanding, governed by the same laws?—The position is not only unsound, but preposterous; an outrage on the immutable principles of causation. The same physical laws, operating uncontrolled, always perform the same processes and produce the same effects. When this ceases to be the case, then may we expect the streams to flow upward, and ponderous bodies to fly from the earth into the heavens, by the power of gravitation.

If the animo-vegeto-chemical philosophers will define their science to be, a series of changes among the elementary particles of living matter, governed by laws possessing different powers, operating under different circumstances, and producing different results, from the laws of the ordinary chemistry of dead matter, then shall I cease to be their opponent, in fact, although I shall still contend, that their nomenclature is unfortunate; because it tends to the propagation of error. But as long as they continue to identify it, in the slightest degree, with the common chemistry of their laboratories, I shall deem it my duty to expose their fallacy, and counteract the contaminating influence of their opinions, by every fair expedient I can

devise, and every honest exertion I can make. And, without pretending either to the spirit of prophecy, or any uncommon degree of sagacity, I fearlessly tell them, that their hypothesis will be dissipated, by the increasing lights of science, as the shadows of night retreat from the sun.

It would be easy to show, did time permit me to dwell on the subject, that, far from co-operating in the same scheme, and to the same purpose, chemistry and vitality are, as already intimated, the very opposites of each other; that they are, in the wise and beautiful economy of creation, antagonizing agents, related to each other as attraction and repulsion, or centripetal and centrifugal forces.

The province of vitality is to build up the world of living matter, and bestow on it the countless forms, beauties, and excellencies which it so abundantly exhibits. Chemistry, on the contrary, pulls down and demolishes these forms, resolves them into their original elements, and thus prepares them for new and different organizations, under the plastic and powerful influence of its antagonist. Hence, no sooner does life forsake an organized body, than chemistry invades it, makes it its own, and reduces it to earthy matter, or scatters it in air.

As relates to the vegetable kingdom, in our own climate, spring, summer, and a part of autumn, are the period in which vitality is busy in its operations. During this time it embellishes and enriches our plains and fields, and meadows, and gardens, with all that is gay and gorgeous in foliage and flowers, all that is delicious and valuable in fruit. But, with the last weeks of autumn, comes a frost, "a killing frost" which disrobes the vegetable kingdom of its beauties, and strews them on the ground. Now commence the orgies of chemistry, in the course of which, flowers, leaves, and stalks, with an abundance of fruit, are all disorganized, diffused through air, in the form of gas, or reduced to a loose, earthy, and unsightly mass, adapted to the further purposes of life. Thus, in the wise and beneficent economy of nature, do these two antagonizing powers reciprocate benefits, and contribute abundantly to each other's employment. Vitality constructs animal and vegetable forms, that

chemistry may dissolve them; and chemistry dissolves, that vitality may reconstruct.

But, true and obedient to the ordinances of Heaven, these two powers never *in fact* invade each other's rightful province. It is only in the visionary system, and unfounded report of the "chemico-physiologist" that such invasion is alone to be found. Where vitality works, chemistry is idle; where the former speaks, the latter is silent.

The oak and the gigantic Adansonia digitata live and flourish a thousand years, the pride of the forest. During this long patriarchal period, chemistry never attempts to molest them. They are monuments exclusively of the reign of vitality. But they die through age, or are overthrown by the tempest. Then do the workings of vitality in them cease. They become then the property and domain of chemistry, and are resolved by it into masses of vegetable mould.

Of animal nature the same is true. The elephant lives centuries, and some of the cetaceous tribes as long perhaps as the oak or the Adansonia. During this time they are above the reach of chemistry. But death at length overtakes them, and all the changes of vitality cease. They become now striking examples of the reign of chemistry. Instead of digestion, assimilation, nutrition and secretion, putrefaction is the only process they manifest. Such is the destiny of the entire world of animals and vegetables. Governed for a season by the principles of vitality, they fall, at last, under the dominion of chemistry, and pass into elements.

To render this case the more striking and conclusive, warm-blooded animals are in the very condition best calculated to invite putrefaction. Their temperature is suitable, and they have, in their systems, the proper mixture of solids and fluids. Still, while their economy is the result of vital principles, neither in health nor disease does putrefaction occur in them. In sphacelation, the parts putrefy only when their vitality is completely extinct.

Still further to evince the absolute opposition between vitality and chemistry, the former not unfrequently checks the latter

in its course. A vulture, a hyena, or a famished wolf has feasted on putrid animal matter. In the course of an hour examine the half digested contents of its stomach, and you will find them perfectly free from taint. Their offensive odour is gone, with every other phenomenon that speaks of putridity.

A snake has swallowed a rabbit, a squirrel, or some other small animal. Owing to the difficulty created by the hair of the animal, and its not being masticated, it is digested slowly. From its being too large to be all contained, at once, in the stomach, or real digestive apparatus of the snake, a considerable portion of it remains in the gullet. Long before digestion is completed, that portion, which has not yet entered the stomach, and is not, therefore, subject to any vital process, becomes putrid. But, as soon as it makes its way into that organ, and receives the influence of its digestive function, its putrefactive taint is removed.

In these instances, the vital process of digestion takes an ascendency over the chemical process of putrefaction; an ascendency perfectly in the order of nature, the vital being confessedly the more elevated power. Other cases of a similar character might be easily cited.

Of those writers and teachers, who inculcate the hypothesis, that certain functions of the animal economy are performed by chemical agency alone, my notice shall be brief. The belief is so unreasonable, not to say absurd, that there are very few, at present, who make an open avowal of it. Some chemists do, indeed, still continue to talk and write about the chemical carbonation, decarbonation, and oxygenation of the blood, the superiority of the arterial to the venous blood, in its capacity for caloric, and the generation of heat, by the conversion of fluid into solid matter; and the reverse. All this they do, as they would induce us to believe, to instruct us in the production of animal temperature; to persuade us that it is, in some way, the result of chemical agency. But their handling of the matter is so miserably loose, misty, and without point, that it is impossible to determine, to which class of pseudo-inquirers they belong, the purely chemical, or the chemico-physiological; or whether, like a certain equivocal animal, the siren lacerta, they do not bear a

kind of amphibious being, subsisting, by turns, on simple error, in one school, and compound absurdity, in the other.

To those chemists who may contend that any one of the functions of living matter is performed by means exclusively chemical, I beg leave to propound a few interrogatories.

Can you perform any one of those functions in your laboratory? Can you there convert common alimentary matter, well masticated and imbued with saliva, into chyme, the first step in the process of digestion? Can you convert chyme into chyle?—chyle into blood?—blood into muscle, tendon, or bone?—or can you form, from blood, bile, saliva, pancreatic juice, or any other secreted fluid? To each of these questions the answer of the chemist is necessarily, no. I continue my interrogatories. Here is a bone fractured, and a muscle divided by a cutting instrument. Can you, by chemistry, effect a reunion? Again I receive from him a negative answer. My reply to him is, then is your hypothesis unfounded and visionary, or you are uninstructed in the art you profess. You say that by chemical agency alone food is digested, blood composed, and the processes of nutrition and secretion effected. You are, by reputation, a chemist; yet you are unable to perform, by your art, the simplest of those functions. The dilemma is thus prepared for you; and on one of its horns you must necessarily hang. Confess, then, that your theory is false, or yourself ignorant.

But, perhaps, you can succeed better, as relates to the functions of the vegetable kingdom. Let us try. Your chance shall be a fair one.

Can you, from the sap of the rosebush, produce the beauty and fragrance of its blossom?—no—from the juice of the myrtle, the leaves and flowers, by which the plant is adorned?—no—from the sap of the pomegranate tree, the beautiful and fragrant fruit which it bears?—no—or can you, from the life-blood of the peach tree produce even the gum which oozes from its stock?—still is the answer, no. What, then, can you do, in the performance, by chemistry, of a single animal or vegetable function?—Literally, not an act—you can simply talk about it, and nothing more. You can deceive those, who are as ignorant

of the functions and laws of vitality as yourself; and thus are your boasted pretensions interpreted; and here do your art and mystery end.

But, by way of a reciprocated puzzle, or for some other purpose about as valuable, I am asked in turn, what is to be understood by the term *vitality?* I answer, vitality is to be interpreted in the same way as chemistry. They are both abstract terms, each being the representative of a peculiar set of phenomena or changes, in the material world, governed, of course, by peculiar laws. The same is true of the word mechanics. It denotes, in like manner, an insulated and specific set of phenomena, governed by laws peculiar to themselves.

The word chemistry is the symbol of all the facts and phenomena that belong to that science, together with the causes, to which they are attributable, and the laws that control them.

So, in like manner, is vitality the symbol of all the peculiar facts and phenomena, observable in the economy of living organized matter, with a similar reference to their principles and laws. I need scarcely add, that the phenomena of chemical matter, strictly so called, are as different and distinct from those of living matter, as are the phenomena of the mineral from those of the animal and vegetable kingdom. No enlightened and unprejudiced observer of nature will ever confound them.

But I am again, in the unprofitable spirit, not of solving difficulties, but of attempting to divert attention from one difficulty, by meeting it with another, asked, what is to be understood by the principles and laws of vitality; or, in simpler language, what those laws and principles are?

I answer, that in a paper, like the present, it is not permitted me to attempt an explanation of them. Such a disquisition could not be compassed within the limits, to which I must confine myself. To do it entire justice, a volume would be scarcely sufficient. But I fearlessly add, that they are as well settled in science, and as accurately understood, by the cultivated physiologist, as are those of chemistry by the votaries of that science, or those of mechanics by the natural philosopher. From the mere chemist, whose views are confined to his closet and his

laboratory, and who has never made life and its functions his study, they are necessarily concealed. Nor, had he the least discretion, would he ever speak of them, either as an assertor or a denier of their existence; nor for the purpose of giving an exposition of them; in which case, he would escape the disgrace to himself, and the mortification to his friends, of exposing his ignorance, and perpetually burying himself in blunders with respect to them.

As connected with the present discussion, another topic, of no common interest, claims, for a moment, our serious attention.

It is asserted by some chemists, and perhaps by others who are not chemists, that out of a mass of matter, no substance can be produced, either simple or compound, which did not *formally* exist in that mass, possessed of all its peculiar qualities. That if, by a series of chemical processes, iron and sulphur can be extracted from the human blood, the fact amounts to proof incontestable, that iron, possessed of all its metallic, and sulphur, of its mineral properties, were actually and formally in the blood, as it circulated in its vessels, a living fluid. And that, if by the process of combustion, potash can be produced from a log of hickory, or maple, the potash must have existed *formally*, a constituent part of the log, marked by all its alkaline qualities.

If this opinion be not signalized by its absurdity, in the midst of opinions most absurd, then am I deceived in my views of it. Its chief advocates are the humoral pathologists, whose object is to prove, by it, that whatever substance can be extorted from the human blood or other fluids, even by the most complicated series of chemical experiments, existed necessarily, in its formal character, in the blood, as it circulated through the system, and in the other fluids, in their proper receptacles. Having an immediate bearing, then, as well on practical as scientific medicine, the hypothesis is peculiarly worthy of our consideration.

Before proceeding, further, in the illustration of this topic, let me here observe, that until subjected to the processes of decomposition, and recomposition, the one by combustion, and

the other by the requisite chemical agents, wood exhibits no sign of alkalescency, nor blood of its holding, in its composition, either iron or sulphur. Of those two compounds, then, these substances are not the native products. They are the result, not of the functions of animals and vegetables, but of the chemical processes by which they are made manifest.

It has been already stated, that the rich and fragrant blossom of the lotus of the Nile, is formed exclusively out of the turbid waters and their slimy sediment of its parent river. But does it thence follow, that the exquisite fragrance, in its actual condition, and possessed of the qualities which render it so precious, pre-exists in those waters? Show me the humoralist who will answer in the affirmative.

The epidendrum aerium, with the beauty and delicious odour of its blossom, is fed and formed out of the atmosphere around it. But no one will contend that the odour pre-existed formally in that atmosphere, in a latent state, awaiting the flower to take it up, set it at liberty, and render it sensible.

Some of the canes, with their rich and finely flavoured saccharine juices, are nourished and produced from the soil in which they grow, and others, as is believed, from the atmosphere alone. But no one imagines that those juices, in their formal state, have an actual existence either in the earth or the air. Such a hypothesis could prevail only within the walls of a mad-house.

Wine is prepared from the grape. The grape is prepared from the sap-juice of the vine, and that from the soil and atmosphere by which the vine is nourished. But who will have the wildness and hardihood to maintain, that the provinces of Champaigne and Burgundy, in France, contain in their soil and atmosphere, the racy and sparkling beverages to which they have given their names?

It has been already stated, that the sugar contained in some of the canes is formed from the atmosphere, and that in others, from the soil in which they grow. From sugar is produced, by a chemical process, the fluid called alkohol. But no chemical process can procure that fluid from either the earth or the atmos-

phere by which the canes are nourished. Nor has the spirit of hypothesis yet grown so rampant, as to contend that it *formally* exists in them.

Water is one of the deadliest foes of combustion, and is, therefore, employed as among the most effectual means to extinguish it. Yet is that fluid compounded of two gases, one of them itself highly combustible, and the other the universal feeder of combustion; and both of them can be easily extracted from it. Will any philosopher, then, undertake to maintain that hydrogen and oxygen, in the full possession of all their properties, actually and formally exist in their compound called water? Folly itself will shrink from the absurdity of an affirmative reply. But I retract the expression. There is folly so consummate that it does not shrink from it. The reply has been virtually given in this place. I myself have been repeatedly the "wonder-wounded hearer" of it.

From two varieties or, perhaps, species, of the Peruvian bark are procured, by a chemical process, quinine and cinchonine, and from the bark of the cornus floridus, the article called cornine.

But I am far from being yet convinced, either that those substances exist formally in the barks, from which they are derived, or possess their native properties. On the contrary, my present persuasion is that they do not. They are chemical compositions, not native products.

If we extend our researches into the animal kingdom, we shall find there matter in abundance equally subversive of the hypothesis I am examining.

Chemists inform us that the muscle of an ox is composed chiefly of oxygen, hydrogen, azote, and carbon. But they will not hazard their reputation by contending, that, in that portion of solid flesh, when possessed of life, and performing the function of voluntary motion, either of those constituents exists, in its formal condition, characterized by its qualities as an elementary substance. Nor will they allege that the viper finds, in the simple fare, on which it subsists, its deadly poison already formed.

In like manner with that of the foregoing, the explanation of the following facts sets chemistry at defiance.

The effect of nitrate of silver, taken for some time internally, is to blacken the skin. The belief of chemico-physiologists is, that this is the result of an impregnation of the blood by that metallic salt, and a deposition of it in the skin. Yet from the blood not a particle of it can be procured, by any process to which that fluid can be subjected. Nor is it found in the skin, to which it has imparted a dark colour. The effect arises from a change in the secretory action of the skin, produced sympathetically, by the direct action of the nitrate of silver on the mucous tissue of the stomach.

Put an individual, either in health or disease, on a course of sulphur, and, in a few weeks, so impregnated will be his perspiration, by that mineral, that it will blacken the silver which he carries in his pocket. Yet is there, now, no more sulphur *formally* in his blood, than at any other time. Nor, can any more be procured from it by chemical agency.

The same is true, as respects iron, after a long course of chalybeate medicine. None of that metal is *formally* in the blood; nor can it be extracted from it, in larger quantities, by means of chemistry, than in cases where no chalybeates have been taken.

In the milk of females, and in the perspiration and urine of every person, who have swallowed large quantities of asafœtida, garlic, or spirits of turpentine, the odour of those articles becomes strongly perceptible. But, in their blood, not the slightest degree of the odour exists. That fluid retains its natural smell.

The explanation of these phenomena, I say, sets all chemical laws at defiance. By the laws of vitality alone can it be effected. They prove incontestably, that, out of compound substances, may be formed many matters, which did not formally exist in them.

To this class of preposterous opinions belongs that which proclaims ice to be nothing but water in its *natural state.* And certainly an opinion *more* preposterous, the human intellect can scarcely imagine. Ice can indeed be formed out of water, and water out of ice. They are, therefore, convertible into

each other. So is rich vegetable mould convertible into a cabbage stalk, and a cabbage stalk re-convertible into mould. A pig, by digestion, assimilation and nutrition, may be converted into a man, and a man, by the same process, into a pig. Yet they are not the same. Vegetable mould is neither the natural state of cabbage, nor cabbage of vegetable mould. Nor, although there are men as stupid as pigs, is a pig the natural state of a man. Yet as well may identity be predicated of them, as of ice and water.

According to the showing of chemists themselves, these two substances are, in a high degree, different from each other.

Water is a compound substance, consisting of certain given proportions of oxygen, hydrogen, and caloric. But it is a law of chemistry, of which many confirmatory illustrations might be adduced, that, by varying the proportions of elementary substances, you change essentially the nature of the compounds into which they enter.

Ice consists of oxygen and hydrogen, with a smaller proportion of caloric than water contains. It is, therefore, a compound substance essentially different from the water out of which it is formed. It is different in proportion to the inferior quantity of caloric it contains. Give to it the amount of caloric which exists in water, and you reduce it to water.

Are the proto-chloride and the deuto-chloride of mercury the same? We know they are not; nor does any chemist so consider them. Why? Because they contain different proportions of chlorine. The former contains one part of that substance, while the latter contains two. Yet are they easily convertible into each other. Add to the proto-chloride another part of chlorine, and it becomes a deuto-chloride; subtract from the deuto-chloride one part of chlorine, and a proto-chloride is the product. As well, then, may a proto-chloride of mercury be pronounced the natural state of the deuto-chloride, as ice the natural state of water. Of this subject innumerable other illustrations might be adduced. And neither in the proto-chloride, nor the deuto-chloride of mercury does there exist either chlorine or mercury in a *formal condition*. The simples are entirely lost in the compounds which they form.

Let me entreat the advocates of the opinion I am opposing, to recollect, for a moment, and seriously consider, how exceedingly limited and superficial is our knowledge of matter. We know of it nothing but its properties. Of its substance we are ignorant, and must for ever remain so, because we have no faculties adapted to the acquisition of a knowledge of substance. Speculations, as to the nature and character of that, as well as in relation to all essences and entities, are but empty visions, and to us transcendental.

Remove from matter all its properties, and, as concerns us, you extinguish it. Change the properties, and you change the matter. To us it is no longer the same, but different, according to the new properties bestowed on it. To illustrate this by examples.

Take from a piece of gold its figure, size, solidity, colour, ductility, and weight, and, to us, the gold has no existence. Take from a peach or a pomegranate, the same properties, together with its odour and sapidity, and the fruit is annihilated.

Give to a peach or a pomegranate the figure, size, solidity, colour, weight, sapidity, and odour of an apple or a pear, and you convert it into one or the other of those kinds of fruit.

Bestow on gold the solidity, weight, ductility, and colour of silver or lead, and silver or lead is the product of the change. Of every other kind of matter, the same is true. You annihilate or transmute it, according as you annul or change its properties. To us, essence, substance, and entity are but abstract terms; the mere symbols of our own conceptions, and nothing more. They represent nothing but an imaginary substratum of qualities, whose influence alone gives us a belief in the existence of that substratum. Remove or extinguish the qualities, and, to us, the substratum is also extinguished. Change the qualities, the substratum is changed.

Identity of property, then, constitutes, to us, identity of substance, and diversity of property, diversity of substance. If to this proposition an exception exists, the knowledge of it would be gratifying to me.

But in the fundamental properties of *solidity*, and *weight*, as well

as in the less important ones of colour and texture, ice and water are essentially different. To pronounce them the same, then, is a palpable violation, at once, of reason and common sense. Both physically and chemically their difference is obvious. As consistently with truth may it be asserted that blood is the natural state of the muscle, or the sap-juice of the pear tree, of the pear into which it is converted, or the reverse, as that ice is the natural state of water. Finally, as well may it be contended, that oxygen and hydrogen, out of which water is formed, constitute the natural state of that fluid; or, that water constitutes the natural state of the two gases that form it. View the hypothesis as you may, it is infinitely preposterous. Yet is it gravely announced, by writers and teachers, as a chemical axiom.

Were it true, that in the mutations of matter, the product is always and necessarily identical with the substance, or mass of substances, out of which it is formed, the result would be inconceivably monstrous and ludicrous.

We, the inhabitants of the West and South, would be but masses of pork, hommany, and cabbage; our fellow citizens, of of eastern and middle states, compounds of beef, flour, butter, and buck-wheat cakes; while the hardy and intrepid sons of New-England would be chemical compositions of cod-fish and molasses. The Englishman would be roast-beef and strong beer, the Irishman a huge and sound hearted potatoe suitably garnished with curds and whey, the Frenchman a mixture of bread and soupe-maigre, the Spaniard and the Portuguese an overgrown onion swimming in olive oil, and the Chinese and the Hindoo an aggregate of rice. Under such circumstances ancient fable would be perfectly beggared by existing reality.

Shall I be asked, whence it is, that, in the various mutations, which, in the economy of nature, matter sustains, the product is usually so exceedingly different from the substances out of which it is formed? I answer, I do not know.

I do know, however, that there are two processes in perpetual performance, by which the character of substances is essentially changed. These are *composition* and *decomposition;* the conversion of simples into compounds, and the reduction of com-

pounds into their elementary parts. By those who speak the language of observation, it will not be denied, that, in entering into composition, elementary substances lay aside the properties which they possessed, as simples; and that they resume them, as soon as the composition is dissolved. In sound philosophy this is recognized as a law of nature. It will be observed that, in speaking of "simples," I use the common phraseology of the day, without vouching that the substances, so denominated, are simples. It is sufficient for my purpose that they are *comparatively* so; being more simple than the masses which they help to constitute.

Thus, when sulphur and oxygen unite in the formation of sulphuric acid, they both put off their specific characters. The compound contains neither real sulphur, nor real oxygen. The former has lost its combustibility, and the latter its capacity to sustain combustion, and support animal life. Decompose the acid, and its elements resume their properties, as simples. The sulphur is again combustible, and the oxygen respirable.

When iron unites with oxygen, to form an oxide, the mutation is of the same description. The iron loses its metallic character; is neither lustrous, malleable, nor of its own specific gravity; nor does the oxygen retain its properties as a simple. In the oxide of iron no real iron exists. Decompose the oxide, and its elements take back their primitive properties. The process of making iron consists in a decomposition of ore, to restore to the metal its true character.

The same is true of the union of soda and sulphuric acid. Each ingredient, in uniting with the other, lays down its characteristic properties. Hence the compound is denominated a *neutral* salt; a substance, whose very name indicates its destitution of the properties of the elements composing it. Decompose it, and the properties of its elementary ingredients are restored.

Of all compound substances, where a real union takes place between the elementary portions, the same is true. The simples resign their properties, on entering into the union, and re-

sume them as soon as the union is dissolved. This is one of nature's axioms.

Wherever, then, the processes of decomposition and recomposition go on, with great activity and force, corresponding mutations in matter are the issue. This is true, whether the processes be under the influences of chemistry or vitality. In the latter case, however, the changes are most striking. In no instance does there occur an actual creation; a genuine production of something out of nothing. Yet so extraordinary, at times, are the mutations effected, that they have been so denominated. At least they have been pronounced a positive generation.

Thus, in the work called "The Book of Nature," by the late Dr. Good, (see American edition, pp. 149–150,) we find, on this subject, the following paragraph.

"I have already had occasion to observe, that albumen and fibrine are substances formed by the action of the living principle out of the common materials of the food, and that it is probable the lime found in the bones and other parts is produced in the same manner. Whether the iron and sulphur that are traced in the blood have a similar origin, or exist in the different articles of our diet, and are merely separated from the other materials with which they are combined, is a physical problem that yet remains to be solved. It should be observed, however, that the sulphur does not exist in a free state in the blood, but is only a component part of its albumen. Considering the universality of these substances in the blood, and the uniformity of their proportions in similar ages, temperaments, and habits, whatever be the soil on which we reside; that those who live in a country in which these minerals are scarcely to be traced have not less, while those who live in a country that overflows with them, have not more, it is perhaps most rational to conclude, *that they are generated in the laboratory of the animal system itself by the all-controlling influence of the living principle.*"

The same appears to have been the opinion of Dr. Marcett, when he found that chickens, on which he experimented, discharged, in their fæces, a greater amount of calcarious matter, than he could extract from the food on which they lived.

Of the truth of this I entertain not a doubt. The iron and sulphur found in the human blood are not only, by the influence of the vital principle, generated from alimentary matter, in which, as a part of it, their elements had existed, but are retained, by it, in a latent, informal, and inoperative state, united to some other substance, as long as the blood continues to circulate, a living fluid, in the vessels that contain it. They are then mere constituent parts of the fluid, possessing none of their peculiar and characteristic properties. Nor can they possibly be clothed in those properties, until the blood is drawn from the vascular system, and suffers decomposition, either as a consequence of its death, or by chemical agency. Then they appear in a formal condition, endowed with their specific and characteristic properties. Of other substances, found in the blood, the same would seem to be equally true. They are generated, by vital influence, from their elements contained in alimentary matter. This is probably the case with phosphorus, ammonia, soda, and chlorine, all of which are said to be found in the blood. Indeed, as already mentioned, the entire mass of blood is as different from that out of which it is formed, as any one compound substance can be from another.

I am perfectly aware of the opposition, and perhaps denunciation, which I am to expect from chemists, for speaking of the *elements* of iron, sulphur, phosphorus, and chlorine. These, say they, are themselves elementary substances, and cannot, therefore, be composed of elements.

A few years ago they said the same of the alkalis. But time and research have convinced them of their mistake. Nor, as will appear hereafter, are they less deeply mistaken, in other hypotheses, respecting which their present confidence appears unbounded.

As relates to the real elements of matter, they know nothing. Nor is it, perhaps, possible they ever can. The study of *elements*, like that of *substances* and *entities*, appears to be transcendental. Man does not seem to have any intellectual faculty in unison with it.

Judging from the various substances contained in the com-

position of *air-fed* and *water-fed* plants and animals, and which are derived exclusively from their matter of nourishment, there is reason to believe, that chemists know as little of the positive elements of the atmosphere and of water, as they do of gold and platina.

What are elements to chemistry are not so to nature. This has been too often proved to be now held doubtful. Hence the impropriety of the confidence, with which chemists assert their dogmas, as to the number and character of material elements.

There is nothing of which they speak more dictatorially and positively, than of the composition of the atmosphere. Yet from many of the preceding facts and observations, it appears very clearly, that they know nothing about it. Nothing, I mean about its *real and radical elements*. And this view of the subject is still further confirmed, by another fact, which is worthy of our attention.

The aerolites, or meteoric stones, which occasionally descend among us from above, are now believed, not to be of lunar production, nor substances ejected from terrestrial volcanos, but to be formed, by concretion, in the upper regions of the atmosphere.

But those concretions consist of several substances, not one of which chemists have been able to detect in atmospherical air.

Some of these masses, that have fallen from the heavens, are composed exclusively of two metals, iron and nickel. But they are formed generally by peculiar combinations of iron, nickel, silica, and magnesia; to which are occasionally added, small portions of sulphur and lime.

These substances, then, must exist, in the atmophere, either *formally*, or in an elementary state.

But *formally* they cannot exist there, because they are fixed, and not volatile. By no operation, with which we are acquainted, can they be rendered aeriform and sufficiently light to float in the atmosphere. Out of the real constituents of the atmosphere, then, must they be generated, by one of those processes, which nature performs, but of which man is ignorant.

This fact alone, independently of others, that have been previously stated, should prevent chemists from speaking familiarly and confidently of the elements of matter, and of the composition of the atmosphere, as if they were really and intimately acquainted with them.

On the origin of the lime found in the bones and other parts of certain animals, a hypothesis is entertained which merits our attention. It is in the case of that extensive family of the feathered kingdom, that have a strong muscular stomach, lined with a dense and insensible membrane, and denominated a gizzard. Our turkies and common domestic fowls are members of this family.

These animals are known to be in the habit of swallowing gravel, of which, if they are deprived, they become sickly, their eggs are not covered with firm shells, their bones sometimes perhaps deteriorate, and, in a short time, they die.

In explaining all this, the naturalist and physiologist find no difficulty. Nor does the chemist either, in his own estimation. He sees it in evidence, "clear as proof from Holy Writ," that the gravel which his chickens swallow, although he never examined it, is calcarious, and that from it alone is derived the lime which enters into the composition of their eggshells and bones. Withhold from them their gravel, and you deprive them entirely of their source of lime. Hence their bones suffer, their eggs are covered by nothing but a membrane, and the poor things die of a malady called the *mopes*. Such is the moping account of the matter, which the chemist gravely communicates to the world.

An opinion more unspeakably silly than this, has never been believed in, by the silliest of men. There is not a notable housewife in the country, who vigilantly superintends the rearing of her poultry, who cannot correct it. And yet it is solemnly palmed on the public as *philosophy*, and defended with a zeal and dogged perseverance, which should be manifested only in a better cause.

True, our chickens and turkies swallow gravel, sometimes calcarious, sometimes argillacious, and, at other times, silicious, according to the mineralogical character of the country where they subsist. And, provided it be hard enough, one kind suits them as well as another. But from neither do they derive a particle of nourishment, to be manufactured into bones, egg-shells, or any other article. It is intended exclusively for the purposes of mastication. Our poultry cannot digest the corn, wheat, oats, and other hard grains they swallow, unless they be triturated. But they cannot triturate or break them with their bills. They, therefore, swallow gravel, with which, as with grinding teeth, their strong muscular stomachs, bruise and break to pieces the grain, after it has been sufficiently macerated in their crops. When deprived of gravel, and still fed on hard grain, their food is not digested. The consequence to them is precisely what it is to ourselves, under similar circumstances; gastric derangement, imperfect digestion, defective and vitiated secretions, disease of the whole system, and death.

Farmers know well the consequence of feeding an old horse, that has lost his teeth, on nothing but hard, unchopped corn. The grain passes through him undigested, he loses his flesh and strength, and ultimately sickens and dies. But he does not suffer for want of lime to form his bones; although he needs that substance as much as chickens. He dies from a general want of sustenance.

In the maritime districts of the Atlantic states, the poultry are as healthy, vigorous, and perfect, their bones as hard, and their egg-shells as thick and firm, as in the valley of the Mississippi. Yet in the former region there is no calcarious gravel, and in the latter scarcely any other kind.

The eagle, the condor, the hawk, the kite, the common vulture, and other birds of prey, swallow no gravel of any sort. Yet have they sound health, bones of great strength, and perfect shells to their eggs. Even the humming bird, that banquets on nectar, gives to its egg a calcarious covering, and has lime in its bones.

It is singular that some of our dreaming chemists do not push

their absurdity so far as to contend, that birds swallow gravel corresponding in colour to that of their eggs. It is as easy to secrete lime, without having swallowed it, in a formal condition, as colouring matter. Fowls and birds, therefore, whose eggs are reddish or speckled, should swallow reddish or speckled gravel, those with white eggs, white gravel, with blue eggs, blue gravel, while such as have shells or bones, as is the case with some, of a very dark colour, should be furnished with fragments of the black marble of Egypt, Italy, Ireland, or some other place.

Still further to baffle and expose the dreams of chemistry, on this subject, we are assured that, from the body of the terebellum marmoreum, which, as already mentioned, subsists exclusively on marble, no calcarious matter can be extracted.

Feed a turkey, or a common domestic fowl on meal or well chopped grain, giving it neither gravel, of any kind, nor lime, in any other form, and it will remain healthy, become fat, and have eggs covered with shells.

Feed three fowls or turkeys on the same kind of hard grain, giving to one of them silicious gravel, to another argillacious gravel, and to the third pulverized lime, without any gravel at all. The two first will digest their food well, and will be healthy and vigorous, while the last will have indigestion, and will sicken and die.

Were it necessary for our poultry to eat calcarious gravel, to procure lime, for the purposes of their economy, the same necessity would be imposed on ourselves, our domestic animals, and indeed on all animals that are possesed of bones. Lime is as essential to us, to form our osseous system, as it is to our chickens, to form their eggshells. But we procure a sufficiency of that material, without eating calcarious gravel, because we can masticate without it. And so can they, provided they masticate with gravel of any other kind.

Feathers, beaks, hair, horns, hoofs, and claws, are found, by analysis, to contain, in their composition, a portion of ammonia. It is not, however, necessary, to cram our chickens, ducks, horses, cows, and cats, with boluses, or to drench them with potions, of

ammonia, to enable them to form those external defences and weapons of battle. Nor is it requisite for the musk-ox to eat food containing musk, to enable him to secrete that substance from his blood.

Yet as well might chemists contend for these necessities, as for their silly hypothesis, respecting the lime in the bones and eggshells of our poultry. There is, indeed, no absurdity too gross for such visionaries to embrace.

Between the nature of the substances, then, that are found in the fluids and solids of vegetables and animals, and that of the substances on which they subsist, there exists necessarily, as previously stated, no identity. On the contrary, they are always marked by a very striking difference. Nor is there any necessary similitude between a secreted fluid, and the blood from which it is formed.

Vegetables, I repeat, that spring from a calcarious, a chalybeate, or a silicious soil, afford, on analysis, no greater proportion of lime, iron, or silex, than those which grow in situations of the most opposite character. Plants that subsist exclusively on air and water, consist, as already mentioned, of the same elementary ingredients with those that derive their nourishment from the earth. The solid and fluid portions of herbivorous and carnivorous animals consist of nearly the same constituent elements, notwithstanding the different fare on which they subsist; nor, in this respect, do the aquatic and dry-land races materially differ.

In the blood that flows through the emulgent arteries there is no more of urea or the lithic acid, than there is in that which nourishes the brain; nor is there any difference between the blood from which saliva, and that from which the humours of the eye are secreted. In the former there is no salivary, nor in the latter any vitreous or chrystalline matter.

Such are a few of the changes, which are daily occurring in the magnificent pageant of material creation, under the influence of mechanical, chemical and vital principles. Enough, I trust, has been said, to show, that those three sets of principles and their laws are essentially distinct from each other; and that they operate, each set exclusively in its own sphere, without any

mutual mixture or interference. The two remaining vital processes of absorption and intellection, on which time will not permit me, at present, to dwell, shall constitute a theme of future discussion.

A few remarks on the practical mischief of what is denominated "chemical pathology" shall close this article.

Were the advocates of that absurd notion to confine their visions to paper and conversation, they might, in common with other follies, afford matter of amusement. But, when they carry them into the chambers of the sick, they become grievous and alarming instruments of evil. They have produced more individual suffering, and destroyed more lives, than the sword, the musket, and the bayonet combined. The hypothesis, and the practice growing out of it, have assumed, successively, the following shapes.

At one time, fever, scurvy, dysentery, and other complaints, consisted in a general *putrefaction* of the fluids of the body. For this imaginary condition of things, the remedies administered within and without, were *antiseptics*, many of them as unfit and pernicious articles, as man could imagine. This form of practice has destroyed its millions.

The same complaints consisted, at another time, in a general *alkalescency* of the fluids. The practice now, was to drench the patient with various acids, vegetable, and mineral, as long as he could swallow; and then to inundate him with them externally, until he ceased to breathe. This was often a most destructive practice.

Again, the diseases arose from an *acidity* of the fluids; and, to remove that, the sufferers were dosed and irritated, by acrid and nauseous *alkalis* within, and excoriated by them without, until, instead of their original disease, they died of the ignorance and temerity of the doctor.

Another notion represented fever, dysentery, and other morbid affections, as the product of a vitiated condition of the blood, that condition being the result of a chemical process, occasioned by contagion, or some other poison received from without. The mass of morbific matter thus produced, could be removed

from the blood, only, by alexipharmics and sudorifics. Hence arose a course of highly stimulating practice, which, particularly when labouring under inflammatory affections, it was impossible for the human constitution to withstand. I need scarcely add, that this treatment was itself more destructive, than the pestilential diseases, in which it was employed. The human race has been rarely visited by so heavy a scourge.

Some maladies were pronounced to be the offspring of a *deoxygenated* condition of the system. To remedy this morbid state of things, those unfortunately subject to it, or rather to the *supposition* of it, were compelled to swallow, to their great annoyance, and not unfrequently their destruction, huge and repeated doses of *nitre*, because that salt contains, in its composition, an abundance of oxygen. They often drank vinegar and even sulphuric acid, in large quantities, for the same reason.

In the perverted fancy of a certain distinguished chemical pathologist and his followers, pulmonary consumption was the result of a hyperoxygenated condition of the lungs. To cure this, their patients were lodged in cow-houses and stables, bathed in the reeking humidity, and suffocated by the stench of those repositories of nastiness, under the whimsical belief, that their atmospheres, cotaining a diminished proportion of pure air, would deoxygenate the lungs, and restore health. This farce was acted in England, not only within my own recollection, but by a physician, of no ordinary talents and fame, with whom I was several years in correspondence.

Some diseases, arising from an excessive *thinness* of the fluids, were to be cured by *inspissants;* and others, depending on a *laxity* of the solids, were to be removed by *astringents* and *tonics*. The latter notion and practice, present a ludicrous mixture of chemistry and mechanics.

Not many years ago, some of the chemists of France, in analyzing certain portions of the bodies of those, who had died of bilious fever, discovered in them, or imagined they discovered, a deficiency of gelatine. This deficiency was immediately pronounced the cause of the disease. The effect was electric. The report and belief of it spread far and wide, with

nothing less than French rapidity. The consequence to the confectioners of the country was highly profitable, and to the poor calves no less disastrous. The latter were slain, in thousands, to furnish the former with the requisite materials to prepare gelatine, (vulgarly, calves-foot jelly,) as a sovereign reme dy for every modification of bilious fever. Instead of carrying along with him, therefore, a well supplied magazine of tartar emetic, quinine, cinchonine, and cornine, every country practitioner armed himself, now, with jars and gallipots of gelatine, and resolutely poured it into the stomachs of his patients. Nor was he a little astonished, when those uncivil and refractory organs poured it back again, without sending the disorder packing along with it. The effect of such folly, in augmenting disease and suffering, and in destroying life, need not be represented. To every skilful practitioner of medicine, it will appear in relief sufficiently bold. It tended, moreover, to the disgrace of the profession.

Nor are the various lithontriptics, as they are called, which have been so confidently administered, on chemical notions, entitled to any higher commendation. They have never dissolved a single calculus in the kidney, or the bladder; nor will they ever do it, while the present laws of the animal economy continue in operation. But they have done great mischief in the hands of empirics.

Even the practice of surgery is not free from the fooleries of chemistry.

Does a wound or an ulcer show a disposition to sloughing or mortification? The part is immediately enveloped in some famed antiseptic poultice, or kept constantly dripping with an antiseptic lotion. On this puny and miserable local practice the surgeon relies, to the entire neglect of constitutional remedies, which are alone competent to the removal of the complaint. But the follies and evils of chemical pathology are too numerous to be all recited. They would fill volumes.

To conclude then. As the source of Pharmacy, chemistry is essential to the profession of medicine. But in no other respect. As often as it has made its way into physiology, pathology, or

practice, it has spread around itself error, suffering and death. The exceptions to this are so few, as to do no violence to the general rule. They relate chiefly, if not entirely, to noxious agents lodged in the stomach.

From the matter contained, and the sentiments expressed, in this paper, let it not be imagined that I am a foe to chemistry. No opinion could be more unfounded. I am an admirer of the science, and deem it equally interesting and important. In my youth, I cultivated it, for several years, with great ardour, and no less delight.

But I admire it only when it is kept within its proper sphere. There alone am I friendly to it. There alone is it either interesting or useful. When pushed beyond that, and obtruded into another sphere, for which it has neither affinity nor fitness, it loses, at once, its attractiveness and value, and becomes offensive and mischievous.

Confine it to dead matter, and I am its friend and advocate. But I am hostile to all attempts to introduce it into the economy of living matter; because I conscientiously believe, that such attempts are founded in error, and productive of evil.

NOTE.

To prevent misconceptions of my meaning, in several passages of this paper, where I have been compelled, for the sake of avoiding too much prolixity, to speak in general terms, a very brief exposition would seem to be requisite.

I am aware that in the performance of many of the functions and processes of the body, principles purely mechanical are in constant operation.

In effecting the circulation of the blood, the heart, as I have elsewhere observed, acts as both a forcing and a suction-pump. Locomotion generally, as well as the admission of air into the lungs, and its expulsion from them, in respiration, is performed on mechanical principles. So are speaking, chewing, swallowing, and the peristaltic motion of the stomach and intestines. The blood also obeys, to a certain extent, the influence of gravitation.

But in all these instances, and in every other that can be adduced, the mechanical process, although subservient to the vital, is entirely distinct from it. They do not, in the slightest degree, interfere with each other. The one simply commands, and the other, during health, implicitly obeys, each occupying its own rank and station. Vitality furnishes the spring of action, and mechanics the apparatus. But mechanics can have no agency in giving the spring, nor vitality in the form and arrangement of the instruments. Although, therefore, the two sets of principles are essentially distinct from each other, there is no such incompatibility between them, as to prevent them from being concerned in the same function, as principal and subordinate. Perhaps the chief reason of this may be found in the fact, that mechanics are concerned with *masses* of compound matter, and vitality with its *elements*.

But, between chemistry and vitality, no such compatibility exists. They both act on and with matter, in an elementary state. Being essentially different in their principles, therefore, they cannot act together. The presence of the one is compatible only with the absence of the other. Admitted into the same process, they would necessarily clash, obstruct each other's movements, and produce confusion.

This explanation has been added since the reading of the paper, in the Lexington Medical Society, at whose request, as the following note indicates, it is printed.

Lexington, November 29th, 1828.

DEAR SIR,

We have been appointed a committee, by the Lexington Medical Society, to ask of you, for publication in the Transylvania Journal of Medicine, the very able and interesting paper read by you before the Society at its last meeting.

With sentiments of the highest respect, we remain your most obedient and humble servants.

WM. M. GWIN, M. D. of Tennessee.
RICE M'COY, of Michigan.
WM. BOMAR, of Virginia.

PROFESSOR CALDWELL.

ART. II.—*A Case of Preternatural Membrane in the Vagina, requiring an operation during labour.* By WILLIAM HALL RICHARDSON M. D., Professor of Obstetrics and the Diseases of Women and Children, in Transylvania University.

On the evening of the 19th of November, 1824, I was sent for by Dr. Davenport of Nicholasville to perform, as he supposed and believed necessary, the Cæsarian section on a female slave, the property of Mr. David M^cKee of Jessamine county.

On my arrival, about four o'clock in the morning, I found the woman in strong labour, at the full period of utero-gestation, with her third child, which had lasted, with scarce any intermission, from the previous Tuesday. Upon examination, per vaginam, there was to be felt, through a firm smooth membrane, the head, which was the presenting part of the fœtus, not having descended lower than the middle of the cavity of the pelvis. I learned from the attending physician, that her bowels and bladder had been sufficiently evacuated; her pulse was neither depressed nor morbidly excited, there was no heat or dryness of the vagina, or other symptoms of danger or alarm, than the inability to find the os tincæ or the presenting part of the child.

After a diligent examination, I became satisfied that there was an obstruction of the vagina, caused by a membranous septum thrown across the cavity and closely adhering to tbe vaginal parietes; the product in all probability of inflammation, seated in the vagina, subsequent to her second delivery, which was two or three years before.

The nature of the case being ascertained, it then became a question of importance to decide on the correct mode of practice. As the fœtus still manifested signs of vigorous life, and the condition of the woman was by no means alarming, it was determined to await the effects that might be induced upon the membrane by the pressure of the head of the child against it. The action of the uterus kept up with unabated vigour un-

til eight o'clock the next morning, when its powers appeared flag, and to have but little further effect in lowering the uterine tumor. I discovered at each examination made during a pain, that the vagina, exterior to the obstruction, was evidently moistened by a fluid proceeding from within the septum: as the condition of the parts was then more favourable for minute examination, there was discovered about the middle of the tumor, a small circular depression, seated on a ridge or projection of the membrane, extending from the recto-vaginal septum, to the urethral part of the vagina. In the centre of the depression there could be discovered a small round hole, which would perhaps admit the blunt end of a small probe, through which the liquor amnii had doubtless escaped, as there was no collection of that fluid in advance of the head pressing on and in contact with the membrane.

There could not be discovered during the parturient efforts, the least tendency to yield at this point more than any other. Notwithstanding the occiput had reached the arch of the pubis and the face was thrown into the hollow of the sacrum, yet the advance appeared to result more from a distension of the vagina beyond the septum, than from any yielding or giving way of the preternatural membrane.

The examinations gave a very distinct idea of the thickness and firmness of the membrane, which appeared to be between one and two lines in thickness and of a very firm and compact texture. After waiting until about half after 10 o'clock and perceiving no prospect of lesion of the membrane from the pressure of the head of the fœtus, I determined to make an incision through it, and in this way remove the only obstacle that appeared in the way of delivery. For this purpose, I guarded a probe-pointed bistoury, and after placing the woman in the usual posture for delivery, applied the end of my finger, upon the circular depression before mentioned, and then cautiously carried the point of the bistoury to this spot and carefully cut through the septum to the extent of half an inch. My finger was then passed through the incision and came in contact with the head of the child, which was readily recognized. As I was about to

enlarge the incision with the instrument, a pain ensued, and finding the breach in the membrane to part easily by pressing downwards with my finger, it was extended to the rectum, and immediately the remaining portion was divided in the same way to the urethra, without any additional suffering or pain. The head instantly engaged in the breach, and upon passing my finger within the margin of the incision, behind the symphysis pubis, the os tincæ was readily distinguished. There did not follow the operation any discharge of liquor amnii, and but very little blood. The labour immediately became more active, and although it was considerably retarded by the resistance made by the lips of the septum, which surrounded the head very much in the way of a rigid and insufficient dilatation of the os tincæ, so often observed, yet it was completed before twelve o'clock.

The woman was "put to bed" with no other symptoms, than those usual in ordinary labour. The head of the child was large and considerably elongated from pressure, it manifested at birth, no signs of life, but upon the employment of the usual remedies on those occasions, respiration was soon established, and the mother and child left in charge of the attending physician.

REMARKS.

So far as I could collect the previous history of this case, there was nothing unusual during the period of utero-gestation.

The woman informed me that her previous labour was tedious and unusually painful; but could give no other information concerning it, that would lead to a knowledge of the causes that excited the growth of the membranous septum. Subsequent to the second labour, she was not conscious of having ulceration seated in the vagina, or any other disease that would end in the formation of such a membrane. In most instances where there are such alterations in the structure of the vagina, as to furnish obstacles to delivery, they are overcome by the strong pressure of the head of the child, and therefore do not require surgical relief.

This case however, I feel persuaded will be received by the members of the profession, as an exception to the general rule of practice, in similar cases, and the great simplicity of the operation together with the immediate relief of the patient, will authorize a resort to the knife, before the delay proves dangerous to either party, rather than rely two implicitly on those efforts, which may prove ineffectual. Inflammation with its consequences, it was apprehended, would follow the operation and delivery, but nothing of the kind occurred; and the last information concerning the woman and child, received more than a year after the operation, was that they both enjoyed good health.

ART. III.—*Cases and Observations on Dropsy.* By DAVID M. PORTER M. D., late of Paris, Tennessee.

On the 11th of July, 1827, I was requested to visit a family of negroes, six in number, who, the master said, "were all puffed up." It consisted of a mother and five children, four girls and one boy; the latter about the age of twelve, the youngest of the girls about three years old. The mother and four girls were labouring under ascites, with partial anasarca. The pulse of the mother was full, firm, and remarkably irregular as to frequency; countenance dejected, face bloated, respiration embarrassed, abdomen much distended; fluctuation of water very sensible on percussion; appetite almost insatiable;* tongue perfectly healthy in colour, somewhat dryer than natural. She had had no discharge from the bowels for forty-eight hours, and complained bitterly of a severe pain in each knee. The pulse in the four little girls, did not vary materially from a state of

*This excessively morbid appetite was observed in them all.

health; the bowels of all much constipated; the anasarca in each more considerable than in the mother, upper and lower extremities considerably swelled; the labia of each so much swollen, as almost to obstruct the passage of urine, though this was scanty and high coloured in all: in neither did the tongue manifest the least gastric derangement. The boy's case was more aggravated; it was also of the combined form of ascites and general anasarca; his appetite of the same condition with the rest; thirst but moderate; his whole countenance of a bloated appearance, his pulse most remarkable for its irregularity: it was very frequent, though irregularly so, and very small, yet occasionally discovering a full, strong, and tense beat. The urinary secretion very scanty, not perhaps exceeding a gill in twenty-four hours, was of a whitish turbid appearance, and, on being permitted to stand deposited a glairy substance, somewhat of the appearance, and consistency of the white of an egg; his scrotum was so much swelled up with serous effusion, that the penis was quite retracted, and enveloped as it were by it; complained of much dimness of vision, and swimming in the head; fulness about the scrobiculus cordis, with pain on pressure; very oppressive respiration; he said "he felt like there was a millstone in his breast," great soreness and pain over the region of the spleen and kidneys.

The disorder made its appearance in all, about the same time; the mother and all the children, have been uniformly healthy; for notwithstanding they lived in a miasmatic district, they had so far escaped any attack of the epidemic of the country. So far as I could with the assistance of the mistress, trace the genealogy of the family, none of their ancestors had ever been afflicted with dropsy. I do not believe in the hereditary nature of this disorder, but a knowledge of constitutions often aids us in forming a prognosis, and enables us frequently to build our practice on a surer foundation, and sometimes assists us in our investigations into the remote causes of disease. But to return; nothing remarkable occurred in the treatment of the four little girls. Calomel in large doses, in combination with pulverized squill, speedily removed all appearances of watery

accumulation. I say large doses, for the youngest required as much as thirty grains to move its bowels. My first object in the mother's case, was to operate on the bowels. I apprehended some difficulty from the extreme costiveness that was so manifest in them all; *and which was a peculiar characteristic of the fever cases of the season.* She took at once, calomel and jalap thirty grains of each, at 2 P. M. on the 11th.

12th. 10 A. M. Medicine not operated; tongue more moist and slightly coated in the centre; pulse full, frequent and corded; pain shifted from the knees to the hips; other symptoms as on yesterday. Took thirty ounces of blood, which produced little or no perceptible impression upon the heart; gave an ounce of sulphate of magnesia and fifteen grains of jalap in solution, together with an enema to facilitate its operation.

13th. 9 A. M. No operation yet, enema passed unaltered; pulse hard, tense, remarkably irregular in frequency; pain translated to the sacrum. Took twenty ounces more of blood; waited a time to see its effects; slight impression produced on the heart but did not last long; gave twenty grains of calomel and ten of aloes, and ordered the enema to be repeated. At 4 P. M. medicine not operated, enema passed as before unchanged; pulse small, quick, and corded; pain left the sacrum; intensely severe in the right shoulder; respiration hurried and laborious; anxious countenance; not urinated since yesterday, a fact that I had not been apprized of in the forenoon; ordered an enema of salt, aloes and warm soap-suds; it returned in ten minutes unchanged. I knew the life of my patient could alone be saved by a speedy and effectual catharsis. Prejudices run high against calomel in this country. It was in vain I urged the further repetition of this mineral; mistress and patient obstinately refusing to try any more medicine at all. I believed the difficulty in procuring an operation from the bowels was owing to some stricture in the intestinal tube, although there was no pain complained of. I wished to try again the effects of bleeding, but here again I was repulsed. With a view of coercing the lady into measures, I threatened to retire from the case and share none of the responsibility, if I

could not be permitted to pursue such a course as I believed to be right, and she alone would be to blame for the death of the negro; but this artifice proved no less ineffectual than my former intreaties; for she said she would call in Dr.——, a celebrated empiric of the neighbourhood. I knew there was no time to be spent in a parley, and rather than Dr. —— should have any share in the *crop*, whether the case terminated fatally or otherwise, I determined not to shrink from any responsibility; and be the spoil rich or poor, I resolved not to divide it with one who had not even the shadow of a claim to any other distinction in the profession than that of a *quack*. What was now to be done in this formidable case? the most fixed and inveterate prejudices existed against medicines, nothing but enemata were permitted and these yielded to with reluctance, for they had been repeatedly given with no effect. It will, therefore, readily be seen, that I had little latitude allowed me, being exclusively confined to injections. In this extremity I determined to try the efficacy of tobacco smoke. A bladder, with a quill attached to its mouth was procured, and with the aid of a pipe, it was filled with strong tobacco smoke, which was thrown up the rectum. Its effects were no less prompt than gratifying. In less than ten minutes my patient vomited, which was soon followed by full catharsis, and a most copious flow of urine. There was an almost instantaneous mitigation of symptoms; ordered cream of tartar water, to keep up the discharges, and left her.

14th. 10 A. M. Medicine still operating; discharges thin and watery; strength failed some since yesterday; pulse quick and feeble; pain in the right shoulder entirely vanished; respiration full and freer: Left twenty pills composed of aloe. socot. g. scammony, and calomel each forty grains, directed two to be given every hour until full purging was produced: the mistress said she was too weak to be purged; I told her to recollect the situation of the negro on yesterday, and she said no more.

15th. 1. P. M. Had five copious alvine dejections of a dark green colour; urinary discharges frequent and in large quantities; feels much stronger; respiration free and unembarrassed. All prejudices against the medicine removed; pills continued.

16th. 2 P. M. Pills operated well, discharges not so dark, little tinged with yellow; astonishing quantities of urine voided; strength still improving; abdominal swelling sensibly diminished; pills continued. Did not see her again until the morning of the 19th. On asking her how she was, she replied, "the pills have most made me well." She had taken them regularly, with the same happy result as on the 14th, 15th and 16th. She was now nearly reduced to her natural size. I gave her a box of the same pills, with directions to take three night and morning. These pills perfected the cure; no tonics were found necessary; no relapse occurred.

The boy's case presenting symptoms of a more aggravated nature, and being much more rapid in its progress, made me suspect the existence of some visceral inflammation. Took sixteen ounces of blood, gave sixteen grains of calomel combined with three grains of pulverized squill.

12th. Medicine not operated; pulse more regular in frequency and force, yet tense; complains much of pain and soreness as before; other symptoms not materially different from yesterday. Took twelve ounces of blood, gave calomel and jalap fifteen grains each, with directions to drink senna tea.

13th. No operation from the bowels; tea not drank; urine as on the first day; all the symptoms nearly as yesterday. Drew twenty ounces more of blood, which removed for a time the swimming of the head, but produced no other effects. Gave calomel ten grains, aloes soc. eight, and scammony six grains, in four pills, at once, and a saturated solution of the epsom salts, a table spoonful to be taken every half hour until catharsis should follow; placed a large blistering plaister over the region of the kidneys. At 4 o'clock P. M. copious alvine discharges produced; plaister drew well; a quantity of urine voided; dimness of vision, and swimming of the head ceased; pulse full, no longer tense and hard. Made calomel, aloe. socot. g. scammony each forty grains into twenty pills, (the same pills his mother took on the day following,) one to be taken every hour.

14th. Four copious alvine dejections, very black and highly offensive; urine copious, no longer a deposit on standing; scro-

tum subsiding; strength improving; pulse full and regular at eighty in a minute; blister discharges profusely; patient expresses himself better every way. Pills continued as yesterday.

15th. Worse in every respect; but one stool from the medicine; urine scanty and high coloured; pulse hard, wiery and irregular; had a fever before day preceded by a chill; passed a restless night, with frightful dreams; blister nearly dry; pain in the back part of the head; sore throat; he had slept exposed to the night air from a large crack, which satisfactorily accounted for these sudden changes for the worse; drew twelve ounces of blood; gave five pills at once; directed an enema in case they did not operate. At night medicine had operated well, having brought away six copious discharges of thick consistency; the enema was not used; urine as on the 14th; pain left the head; slight pain about the kidneys; none over the spleen; abdomen still tense: directed three pills every three hours.

16th. Much better; the pills operated well; the abdominal swelling beginning to subside; the strength improved; all pain vanished; urine copious; pulse natural at eighty; pills continued as yesterday through the day. Did not see him again until the morning of the 19th; found him setting up and eating; pills had been regularly taken as directed with the happiest result; the scrotum had assumed its natural size and appearance; the abdomen flaccid; the urine natural in every particular; the swelling of the extremities entirely reduced; pulse natural at eighty; directed two of the pills to be taken every night. In ten days his health was completely restored without the use of any other remedies.

Oct. 16th, 1828. Though not regularly engaged in practice, I was requested to see a little girl aged eleven, who had been for some time labouring under ascites, with an evening exacerbation of fever; impeded respiration; bloated, anxious countenance; pain in the left breast; pulse small and frequent; pallid complexion; abdomen much distended; fluctuation of water on percussion quite perceptible; urine scanty, high coloured and coagulating on being exposed to heat under the boiling point; tongue coated over with a thick dark fur; foul breath,

frequent eructations which were highly offensive; appetite had been declining for some time; thirst almost insatiable; has an evacuation from the bowels every day, but clay coloured; some quack nostrum, which no doubt had slain its scores, had been perseveringly given until all hopes of success vanishing, fortunately made the family abandon it. She had been uniformly healthy until the age of seven, since which time she has lived a life of rest and inactivity, being confined principally to needlework. I attributed her dropsical disposition to defective lymphatic absorption growing out of the inactive life she had been compelled to lead for the last four years. This was the best pathology I could form of her case, although it is not very satisfactory even to myself. Much is yet to be learned respecting the physiology of the absorbent system; there can be no doubt however, that this tissue of vessels is capable of high functional excitement, and from this we are warranted in believing that they are subject to morbid and depressed action, and that many cases of dropsy arise from this cause. With this view of the case, after opening the bowels with a mild cathartic, I prescribed digitalis, from which I had every reason to anticipate success from the terms in which Dr. Blackall and others speak of this remedy in similar cases. The preparation used was that recommended by Dr. Withering; viz. one drachm of the dried leaves infused in a half pint of boiling water, of which one drachm was directed night and morning; first dose taken on the night of the 17th.

18th. 1 P. M. No material alteration since yesterday; digitalis continued.

19th. 12 A. M. Still continues about the same. I expected some alteration in the urine, but none was perceptible; still coagulates on exposure to heat. The uncertain operation of this medicine was not overlooked. Four drachms had now been taken without the slightest manifestation for the better. I determined however to give it a thorough trial and risk the consequences. The dose was increased to a drachm and a half twice in twenty-four hours: remained with her this night.

20th. Passed a restless night, broken sleep, hurried dreams;

pulse small, soft and frequent; the distension of the abdomen so great that the skin seems as if bursting; no passage from the bowels since yesterday; no other change worth noting: had the temerity to continue the digitalis; business called me off. At twelve o'clock P. M. summoned in haste to see my patient: it was nothing more than I expected; I found her sorely afflicted with a pain over the right eye, disturbed brain, pulse retarded, palpitations, faintings; I apprehended convulsions would speedily follow. I now for the first time discovered my error; but this was the first signal for discontinuing the medicine. I had now to counteract the effects of this potent article, perhaps too inconsiderately administered. I hesitated as to the propriety of giving opium in any shape, owing to the excruciating pain in the head: thirty drops of the tincture were however given in warm whiskey and water; this was immediately rejected; two grains of solid opium were then given though with much difficulty, for the powers of deglutition were nearly spent; the patient almost insensible; this was however retained; an opiate injection was also given, and a large blistering plaister applied to the pit of the stomach. I had the satisfaction to meet with the happiest result from this practice, all the alarming symptoms above detailed were successfully combatted, and my patient remained *in statu quo.* I was resolved to abandon the whole catalogue of diuretics, and risk the issue of the case upon a well regulated purgative course. Twenty pills made of calomel, aloe. socot., and g. scammon. of each forty grains were left, with directions to take three every night and morning. This course was strictly adhered to with slight variation in the dose for ten days, at which time she had mended most astonishingly, and it was a matter of great surprise to the parents, how she could gain strength so rapidly under the operation of cathartics.

The pills were continued a few days longer, when, the patient being cured, they were laid aside; she is now in better health than she had been for eleven months.

It is worthy of remark that in the progress of this case there was an astonishing number of remarkably large worms passed with the stools, of a character different from any of the known

species. They looked more like the grub in the horse than any thing else I can compare them to, not so large in diameter, but longer, having blunt extremities.

REMARKS.

I was considerably at a loss to know, to what cause I should attribute the dropsy in the negroes. No previous affection had existed in either; there was no feebleness or inactivity of constitution, or in other words, no phlegmatic temperament; but on the contrary they were all remarkable for their buoyancy of spirits and sprightliness of intellect; and if there was any hereditary predisposition, their history discountenanced the belief. I have already stated that they lived in a miasmatic district of country; high-land ponds, in fact, intersperse the whole section of country lying west of the Tennessee river; the streams of water which intersect it in every direction have but very little fall from their fountain-head until they empty into the Mississippi; their banks are very low, and, on every freshet, subject to inundation; of course vast quantities of vegetable matter are annually deposited in the bottoms; the beds of all these streams are remarkably filthy; and in dry seasons large heaps of vegetable matter are left uncovered and exposed to the action of the sun's rays.

I was disposed to believe that these cases of dropsy were nothing more than misplaced forms of fever, and had their origin in miasmata, for the following reasons; 1st. many of the negroes, of the same plantation, had attacks of bilious remitting fever in the same month, obstinate in its character, taking on, in almost every instance, in the latter stages, a typhus form. 2dly. The bowels in all required active cathartics to move them, and they were all cured by very nearly the same course of treatment. 3dly. The dropsical accumulation commencing at or near the same time, (though not attracting the same attention,) must have been under the influence of the same remote cause. 4thly. The whole family being attacked at the same time with dropsy, and four of them so young, (dropsies not being common in such young subjects,) would favour the opinion that it was misplaced

fever, influenced by the same remote cause with the rest, namely, miasmata; for the fever cases could be traced to no other rational source.

But be the remote cause what it might, the cases were dropsy; and the happy issue of the practice, taken in connexion with the case of the little girl, establishes beyond the possibility of a doubt, the efficacy of purgatives in this disorder, given in such varied proportions and combinations as to produce thick consistent discharges. They prove, as these cases clearly show, powerful diuretics in some way; the *modus operandi* I leave for those more fond of speculating than myself, to explain; but certain it is they acted like a charm in removing the watery effusions, and instead of "tearing to pieces, whilst they unloaded,"* actually built up the constitution.

I attach some importance to the injection of tobacco-smoke in the case of the woman; as it may prove in many cases a happy auxiliary in overcoming stricture, or constipation of the bowels, by relaxing the muscular fibre, when other means are prevented; but for it in this case the negro must have fallen a martyr to her mistress' imbecility. There is so much obscurity hovering over the the modus operandi of the digitalis, so many peculiarities and conditions which appear necessary to exist in the system, before we can expect to realize its good effects, if any it has, that I am fully persuaded too much caution cannot be exercised in its employment. From my own limited experience with this plant, and from the multiplied testimony of others, older and more experienced in the profession than myself, I feel warranted in saying, that it should never be given, except the practitioner has it constantly in his power to remain with his patient.

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*Dr. Blackall, in speaking of the efficacy of sup. tart. potass. in the cure of dropsies, says, "The mildness of this remedy, the length of time during which it can be continued in a small dose as a diuretic, the solvent power which it seems to exert over the cause of the distemper, place it in the very first rank:" then he says: "How much more satisfactory and more likely to be permanent is such a cure, than that which tears to pieces whilst it unloads."

ART. IV.—*Essay on the Remote Cause of Typhus Fever, with some Observations respecting the Treatment.* By JOHN ESTEN COOKE M. D., Professor of the Theory and Practice of Medicine in Transylvania University.

THE remote causes of the epidemic diseases which occasionally hurry, in a short time, thousands of the human race to the grave, have long been deeply interesting subjects of consideration with physicians. Although violent disputes have agitated the medical world respecting the origin of these diseases, and in some measure still agitate it, great progress has been made in the last thirty years. The question of the remote cause of yellow fever may be considered as settled, although there are some few who hold on to the belief that it is propagated by a specific contagion; somewhat in the manner of the physicians of Europe in the time of Harvey, of whom not one, it is said, above the age of forty, admitted the truth of the doctrine of the circulation of the blood.

The question respecting the nature of the remote cause of typhus fever is still unsettled. Obscurity rests upon it. It is proposed in the following essay to investigate this subject.

It is perhaps universally believed in England that the remote cause of this disease is a specific contagion, and very many in this country have adopted the opinion. It is proposed to show in the first place that this doctrine is erroneous, and that a specific contagion is not the remote cause of typhus fever.

Contagion is a matter generated by a diseased body, capable, on application to a healthy body, of producing in it the same morbid state, and of causing it to produce contagious matter of the same kind.

Some contagions remain adhering to the surface which generates them: others are volatile, as we conclude from the circumstance of their producing their specific effect without the contact of the healthy with the sick person. Volatile contagions

produce their effect in a manner unknown, but without contact with the sick, or with any thing visibly proceeding from them; but as we know that by approaching near to a person affected with the small-pox, a person who has never had it, will be affected with that disease, we are entirely persuaded that something passes from the sick, and affects him who is exposed to its influence. This unknown effluvium we call a volatile contagion, because it flies through the air from one to another.

As it is very easy to avoid contact with diseased persons, very few people, comparatively, are affected by the fixed contagions; and because very many people are attacked by epidemic fevers, and most of them without having been in contact with the sick, and many of them without having even seen them, these diseases, if popagated by contagion, must be propagated by a volatile contagion.

What evidence have we of the existence of volatile contagion? None, but that which arises from observing certain effects, which we cannot account for in any other way, than by attributing them to an invisible cause which we distinguish by this name. We see for instance, that the symptoms of a disease never appear in a person, without his having been in the presence of some one affected with that disease; and that they are invariably the consequence of being in the presence of a person so affected, without regard to circumstances. Thus, the symptoms of small-pox never have been known to appear in any one without his having been in presence of a person affected with that disease,* and invariably appear when a person is so exposed. Wherever the sick go, they carry the disease; and it never appears where they do not go.* We therefore conclude that something emanating from such diseased person, affects those who go near them.—Obviously then, we have no right to conclude that a disease is produced by a volatile contagion, *when it does not appear* in those who go into the presence of the

*Excepting when the disease is produced by inoculation, in which case the fixed contagion of small-pox is carried to the person affected, and inserted into his flesh.

sick. On the contrary, perceiving that the only evidence of contagious nature that we can have, is wanting in this disease, we must conclude that it is not contagious.

Sometimes the circumstances are such as to mislead those who decide hastily or do not examine closely. Thus we often see persons in good health become sick on going near to those who are ill; but we shall often be wrong in our decision, if we pronounce the disease contagious in every such case. There is a cause for every disease; and that cause is, or has been, in operation when we see the effects appear. The cause may originate in the sick, or in the circumstances in which they are situated. In order to decide the question, the sick and the circumstances should be separated. If when the sick are removed from the circumstances, the disease appears in those who go into their presence, we may conclude that the cause proceeds from them. If, on the contrary, the disease does not appear in those who go where the sick are; but does appear in those who go where the circumstances are, we surely are compelled to conclude that the disease is produced by a cause which does not emanate from the sick; but by one which arises in those circumstances.*

*This error of attributing a disease to a cause emanating from the sick, because those who go among the sick are attacked, has frequently been committed. Those who go into places in which the yellow fever is prevailing, are very apt to be attacked: it does not however follow necessarily that the cause of this disease emanates from the sick, or is a specific contagion; it may be an agent arising in the circumstances in which the sick are situated. If we were to admit the mere fact, that the disease is the consequence of going where people are lying sick, as evidence that the disease is produced by a cause emanating from their bodies, we should be compelled to admit that the common autumnal remittents and intermittents are contagious. On this very ground indeed, Cleghorn asserts that "these tertian fevers have as good a right to be called contagious, as the measles, small-pox, or any other disease; for although in that season, there certainly is a peculiar disposition in the air to affect numbers in the same way, yet those who are much conversant among she sick are most liable to catch the listemper." It is evident that he overlooked the truth, that another cause may be produced in the circumstances in which the sick are situated: and we now know, from the fact that this disease arises wherever and whenever heat and moisture are co-operating upon dead vegetable matter, and ceases when that co-operation ceases, and never appears

There is another test of the contagious nature of a disease. If a disease be propagated by a volatile contagion, those who are in attendance on the sick must take it; this being the only circumstance which convinces us of the contagious nature of the disease. These attendants having been affected must in like manner communicate the disease to their attendants; these to others, and so on until the disease becomes universal, unless by some means or by some law regulating its action, it is checked in its operation. This check is provided, and consists in the incapacity of volatile contagions to produce disease more than once in the same person. Thus, the small-pox cannot affect the same person more than once. The operation of this check is to render those who have had a contagious disease secure from a second attack, and to enable them to attend the sick and cut off the communication with those who have not been affected, and arrest the progress of the disease. Without this restriction, a contagious disease would be perpetual also: for, as soon as a person should recover, he would be liable to be attacked again, and the whole world would be filled with distress.

There is however no universal and perpetual disease, and it

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when this operation is not going on; and likewise from the fact that the disease appears in that direction chiefly, almost entirely, in which the wind blows from the spot in which this operation is going on, that an agent is produced in those circumstances, which is the cause of the disease; and we further know that a specific contagion emanating from the sick is not the cause, from the following decisive test. Separate the sick from the circumstances in which the disease arises and observe the result: where the sick are now situated, we may go with perfect safety: where the circumstances are we cannot; the disease is still produced in those who venture into them, if not an individual be there, if it be on a desert island.

The same is true of the yellow fever. This disease never appears in Philadelphia unless the medium temperature of the atmosphere at three o'clock P. M. in June, July, and August, is above 80° of Fahrenheit's thermometer; and does appear whenever that state of the atmosphere exists through those months. Observe now what occurs in that city and in New York, upon removing the sick from the circumstances in which the disease makes it's appearance. The inhabitants of that district of the city in which this occurs, have been removed and the district enclosed by a fence and guarded. He however who passes that fence, does it at the peril of his life. He who follows the sick to their new habitations, and mixes freely with them is unhurt.

therefore follows that all diseases propagated by a volatile contagion are so restricted in their operation, that they cannot affect the same person repeatedly: and from this it immediately follows, that those diseases that are not so restricted in their operation, that is, those by which we are liable to be repeatedly affected, are not propagated by a volatile contagion.

It has been said that some diseases are contagious in certain circumstances of confined air &c. If there should be such a disease, it must become universal and perpetual *in those circumstances*, unless the contagion which propagates it, be restricted in its operation in the way before mentioned; that is, unless it be incapable of affecting the same person repeatedly. There is however no disease universal and perpetual in the circumstances mentioned; and it follows that all diseases propagated by a volatile contagion in certain circumstances, are so restricted that they cannot affect the same person repeatedly; and consequently those diseases not restricted, viz. those by which we are liable to be repeatedly affected, are not propagated by a volatile contagion.

These are therefore additional marks whereby we can at once decide whether a disease is or is not contagious. One is, if we are repeatedly liable to have a disease, it is not propagated by a volatile contagion. Another is, if a disease be propagated by a volatile contagion, it will spread until it becomes universal, unless its progress be checked by cutting off the communication between the sick and those who are liable to have the disease, by means of those who are not liable to it, in consequence of having had it already.

All the characteristics which have been mentioned are found in the small-pox.

It is never produced without approaching the sick, or applying the contagion procured from them.

In persons placed near the sick it is invariably produced.*

It invariably spreads from the sick to those who are exposed to it,* without regard to circumstances.

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*In some very rare instances it is not thus produced; but this is no exception to the principle, because these persons are not liable to have the disease in

It would spread universally unless stopped by cutting off the communication between the sick and the well who are liable to take it.

We are not liable to be repeatedly affected by it.

We therefore believe that in this disease, something passes from the sick which affects the well, and this we call contagion.

Let us apply these principles in the investigation of the subject of the contagious nature of typhus fever.

It is proper that it be understood that typhus is called by various other names in the works of the English, Scotch, and Irish physicians. Dr. Currie speaks of typhus *or* jail fever.* He says in his Medical Reports, "When the term fever is used in the present work without any adjunctive, it is the low contagious fever that is meant. This is the typhus of Dr. Cullen; the contagious fever of Dr. Lind; febris inirritativa of Dr. Darwin. In popular language it is generally called the nervous fever; and when particular symptoms appear, the putrid fever." "This is the common fever of England," &c.† "Doubtless, the typhus or low contagious fever, is the prevailing fever of this island and of Europe."‡ "It is the epidemic of all our great towns, of our jails, hospitals, manufactories," &c.§

Dr. Pringle says he has observed the same fever which arises in crowded hospitals arise in crowded barracks and transport ships, and that it is the same called in England the jail distemper, and is incident to every place ill-aired and kept dirty.‖

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any way, not even by inoculation. The test therefore holds good to all of the human race who are liable to the disease.

* Currie's Medical Reports, p. 23. † Ibid. p. 27.

‡ Some of our writers have undertaken to say that typhus fever is never observed in this country, and that it is peculiar to the British Islands. This quotation shows that Dr. Currie entertained no such idea. Moreover Trotter in his Medicina Nautica mentions that the French ships captured on the first of June 1794, were overrun with typhus. He therefore did not consider it a disease peculiar to England; and no man ever had better opportunities of observing.

§ Currie's Med. Reports, p. 48.

‖ Pringle on the Diseases of the Army, p. 289, fifth Edition, Quarto, London.

Thus we find that the common fever of England epidemic in the great towns, the typhus, the jail fever, the hospital fever, the ship fever, the putrid fever and the nervous fever are the same; insomuch that Dr. Currie designates the disease by the term fever without any adjunctive.

Although the typhus fever has been so generally, or rather universally, considered as contagious, it has not one characteristic of a contagious disease.

It is produced in certain circumstances, without the introduction of sick persons, or any communication with them or any thing connected with them. The following statements from Currie, Lind, Pringle, and Trotter, fully establish this position. They all believed, it is to be remembered, in the contagious nature of the typhus fever, and therefore no suspicion can exist that the facts were distorted to uphold a theory. The latter had better opportunities of observation than almost any other person that can be mentioned.

Dr. Currie, a decided advocate for the contagious nature of the disease, speaking of a typhus fever which originated at Canterbury, says, "On considering every thing, this cause may reasonably be traced to the crowded and uncomfortable state of their barracks: to the gradual operation of foul air arising from this circumstance, the predisposition to disease is more immediately to be imputed, and finally the contagious fever itself."* "It (typhus) is usually *produced* in situations where there is a want of cleanliness, and more especially of ventilation; and *when produced*, it is propagated by contagion."†

"Dr. Lind informs us, that in a frigate which sailed from North America, with a healthy crew, a malignant fever broke out before her arrival in England, during very bad weather, which affected a considerable number of the men, and of which the surgeon's mate, boatswain, and some others, died." "Thus," he remarks, "a seasoned sound crew became infected, as it

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* Currie's Med. Reports, p. 396.

† Quere, What need of contagion to propagate it? Could not that which produced the first case, produce the subsequent cases?

would appear, from the closeness or damp below, occasioned by the hatch-way being kept shut."*

Pringle says he has often seen this fever arise in crowded hospitals, barracks, and ships; and gives numerous instances.†

Trotter, who, from his situation as physician to the Channel fleet of England in the latter part of the last century, had an opportunity of observing the disease more extensively than almost any other man has ever had, abounds with evidence on this as well as every other point involved in this discussion. Although a confirmed believer in contagion as the cause of typhus fever, he states many cases in which it appeared on board of ships at sea, where it was impossible to trace it to any contagion carried on board. He states that it appeared on board the Niger, the Glory, the Edgar, the Pompee, the Saturn, the Magnificent, &c. and expressly says of each these or similar words, "Its origin could not be exactly traced," &c.‡; and lest it should be supposed that no great care was taken to discover the manner in which the disease was introduced, it is proper to remind the reader that all the fleet believed the disease to be contagious; that Trotter says that a captain who would gladly meet the enemy would turn pale on being told that the fever had broke out on board his ship; that he himself and all the surgeons were constantly on the look-out to prevent the introduction of any thing which could give rise to the disease; and, if a few dirty boys had entered on board a ship, whether they were sick or not, a fever occurring soon afterwards was sure to be attributed to them, as confidently as if they had been known to carry contagion in their clothes. Notwithstanding all this care and anxiety to discover the manner in which the contagion was introduced, it is declared it could not in many instances be traced.

In some cases it was acknowledged there was not even a ground of suspicion that the disease was introduced by contagion;§ and in others that it was generated on board.‖

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* Rees's Cyclopedia, article Contagion.

† Pringle on the Dis. of the Army, p. 22, 25, 63, 287.

‡ Trotter's Medicina Nautica, vol. 1, p. 153, 160; vol. 3, p. 63, 64, 147, 159. 167. §Ibid. Vol. 3, p. 60, 151. ‖Ibid. p. 154, 157, 162.

Dr. Trotter, on a full view of these striking facts, although a decided contagionist, as every man in England then was, makes the following observation: "It is very certain that this fever is generally spread by contagion; but it is equally certain, that it frequently *arises* in places where there could be no suspicion of communication with infected persons or clothing."‡ Considering how perfectly Dr. Trotter, and the English physicians generally, rested in the opinion of the contagious nature of the typhus fever, how perfectly destitute they were even of a suspicion of its fallacy, what great attention he paid to the subject, making the most minute inquiries into the manner in which the disease appeared to be introduced into every vessel, stronger or more satisfactory evidence cannot be desired, that typhus fever originates in certain circumstances, without the introduction of the sick, or of any thing connected with them.

In persons placed near the sick the typhus fever is very often not produced. Very few of those who are near them, attending upon them, are affected with the disease, in proportion to the whole number exposed.

The writings of the British physicians are full of facts which establish this point incontestably.

Dr. Currie in his letter to Dr. Clarke published in his Medical Reports, makes some remarkable statements respecting the Liverpool work-house, which place this matter in a strong light. This was a very large building, containing sometimes 1400 persons, which in the course of the year received from 12 to 18,000 patients. Every person who went into this building entered by a small door at which stood a porter. The person who occupied this post at the time the letter was written, had been in it for several years, and though continually exposed to the operation of any contagion that might be proceeding from the sick who entered, had escaped entirely.

The patients proceeded from the door to certain apartments, one for each sex, where they were stripped and washed, and had a change of cloths; and there they remained until the

‡Trotter's Medicina Nautica, vol. 1, p. 252.

apothecary made his daily visit. Of the people who were employed in this service, *some in the course of years*, were sick.

Moreover all the persons who were admitted, awaited the visit of the apothecary in these two washing rooms; and he decided who had and who had not fever. The patients in fever were put into a ward, or large division, by themselves, and the others were sent to two other divisions of the house; one called the Lock Hospital for females, and the other the Children's Nursery. In ten years the fever did not appear in the Lock Hospital, although almost every person in it must have been in company with the fever patients in the receiving and washing rooms, while waiting for the daily visit of the apothecary: and in the same length of time, there was one season in which a number of the children were affected with fever, and one only; although in addition to the exposure to contagion, if it exist, in the receiving and washing rooms, there was another exposure so remarkable as to excite the surprise of Dr. Currie himself; and enough not only to do that, but to have convinced him that the danger of approaching the sick is imaginary.

He states that the children were allowed to run about the house; and that at the hours of meals, as was very natural in children, they collected in great numbers towards the bottom of the stairs, near the door of the eating room. While Dr. Currie was one day examining the stairs, which he thought badly constructed, a patient in fever was carried in and passed up the stairs within three or four feet of every child on them, and much nearer to some. He was so struck with the exposure and the danger, as he conceived, in consequence of his believing in the contagious nature of the disease, that he immediately counted the number of the children and found exactly eighty-three on the stairs. He also mentioned the circumstance to the nurses, apparently conceiving that it ought not to be allowed; but they considered it a light matter, as what might happen every day. "Though no clear instance of injury arising from such occurrences" he says, "can be brought, I have always deprecated the circumstance which leads to them; and in conjunction with others of my brethren, urged in the parish committee, the pro-

priety of having a distinct passage to and from the fever wards. Various consultations were held on this point; but the structure of the building rendering such an alteration impossible, without entirely defacing the front, the want of any actual proof, or even appearance of injury from using the common stair-case, cooled our zeal." The plan was finally given up. On one occasion a fever prevailed among the children and sixty-seven were sick: what proportion to the whole number this was we are not informed. We are however assured that this was caused by the introduction of the children of a poor family, some of whom had the fever, the children having been sent to the nursery without changing their clothes and with some degree of fever upon them. It must be observed however, that this was said after the occurrence of the fever among the children, by men who were fully persuaded that the disease was contagious, not with a view to decide the question of contagion, for that was no question with them, but to account for the contagion getting access to the children: and they do not assert that the children who were alleged to have introduced the fever, had the disease at the time, but that they had some degree of fever upon them; such a degree as escaped the observation of a man whose special business it was, to take care that a disease believed to be contagious, was not introduced among the other inhabitants of the establishment. This is said too, when in the same letter it is stated that two of the nurses have each of them a child actually living with them in the wards, and going out to school in the day. These children appeared neat, clean, and healthy, though they slept in the very centre of the patients every night.

Lastly with reference to the nurses; they were often and sometimes for a considerable time in contact with the sick, and exposed to the undiluted exhalations from their skin and lungs: and notwithstanding this great exposure, all that is alleged is, that in the course of ten years several "have caught the contagion"; that is to say, several had a fever: for it was the fever that was *observed*, and the supposed cause is evidently used in place of the alleged effect. Accordingly (as might very reasonably be expected) "experience" (says Dr. Currie) "taught them to estimate even this hazard very lightly; and it is in fact

as easy to get a nurse for the fever wards, as a servant for any other part of the house."

What is very remarkable is the statement that, in the year 1801, the master and mistress and a young woman were taken sick and died; which circumstances occasioned great agitation. But, it is stated, they died at different times, and *the master of the house had never been in the fever wards, the mistress but seldom, and they lived in a part of the building very remote.*

Trotter's Medicina Nautica abounds with evidence on this subject. He mentions numerous instances, entirely too many to quote here, in which people having this disease were carried among a ship's crew without spreading it. The following is a specimen of these: speaking of the Charon hospital-ship, he says "We had in that hospital many malignant cases of typhus, and some deaths, yet no infection was ever spread there."*

The following is a striking instance: he says, "We know of nothing that can propagate this fever, but exhalations from the body. But a patient in typhus was sent from the Venerable to the hospital-ship, with a fetor about him, that exceeded any thing of the kind which ever came within my knowledge. After being washed and shifted, it still continued; and was perceived at a considerable distance. He died in a few days; yet nobody was infected from him, either in his own ship or our hospital."†

It would be easy to multiply quotations from Trotter in support of this position; but what has been already stated is abundant evidence that in persons placed near those who have the typhus fever, the disease is very often not produced, or rather seldom is produced.

It is proposed next to show that the typhus fever never spreads even in the presence of the sick, without the concurrence of an impure atmosphere.

This will appear to be true from considering the circumstances in which it is produced, and the result or consequence of separating the sick from those circumstances. This disease is known to be most prevalent in the confined habitations of the

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*Trotter's Med. Nautica, vol. 1, p. 179. See also p. 109, 164, 195, 210; and vol. 2, p. 60, for similar passages. †Ibid. p. 175.

poor in the most filthy parts of cities. Dr. Currie makes some really frightful statements respecting these habitations, and moreover informs us that the streets in which the fever prevails most have numerous collections of standing water, and in those streets which are not drained and in which piles of every kind of filth are allowed to collect, the fever prevails most. Ferriar in his Medical Histories makes similar statements. Remove the sick from those neighbourhoods in which these sources of fever are so abundant, and the disease ceases. Remember the account of the Liverpool work house. From twelve to eighteen thousand patients are carried into it, in the course of the year, from these very streets abounding in moisture and filth, and though the fever wards are in the very centre of the building and cannot be entered but through the common staircase, crowded every day with children, yet no single instance has occurred of the contagion (that is the fever) spreading from them to the other parts of the house.

"It is clear from the collected observations of Dr. Haygarth, and of those active and experienced physicians whose correspondence he has published, not to mention the experience which fever wards have lately afforded, that in a large airy and clean apartment, *few or none even of the most intimate attendants catch the disease*, where the patient labours under infectious fever. The nurses themselves, exposed to the effluvia of the excrements, and perpetually near and often in contact with the sick, nevertheless hardly ever receive infection."*

On the one hand, in the confined air of the miserable dwellings of the poor, situated in filthy and undrained streets, the fever is produced and constantly kept alive, although so many thousands are annually sent into the work house. On the other, in the purer air of this establishment, it is forever expiring; and were it not for the thousands annually sent into it, would cease entirely.

It is therefore evident that this disease does not spread in places into which the sick are introduced, without the concur-

* Rees's Cyclopedia, article Contagion.

rence of circumstances which have been shown to be capable of producing the disease: and when, therefore, we see the disease spread in such circumstances, after the introduction of a sick person, it is not a fair inference that the cause proceeds from the sick; seeing that the circumstances without the sick can produce the disease, and the sick without the circumstances cannot.

Moreover, if typhus fever were propagated by a volatile contagion, it would become universal if its progress were not arrested by cutting off the communication with those in health, by means of persons not liable to be attacked in consequence of having had the disease. There is however no idea entertained of any person's not being liable to repeated attacks, and no attempt made by means of any such persons, to insulate the sick; and yet the disease does not spread until all in a city are affected. Therefore likewise we conclude that typhus fever is not propagated by a volatile contagion.

Lastly, this is a disease to which we are repeatedly liable; and hence also we conclude that it is not propagated by a volatile contagion.

We find therefore that typhus fever is destitute of the characteristics of a contagious disease, viz. that of affecting a person only once; and that of spreading until it become universal, even in the circumstances favorable to its extension, unless stopped by insulating the sick.

Moreover typhus fever often appears in such circumstances, that it is admitted by contagionists that there is not the slightest suspicion of communication with the sick, and it is evidently impossible in some of the cases, from their own statements, that there could have been any: in persons placed near the sick, the disease is very often not produced, and it never prevails without the presence of those circumstances which are sufficient at any time to produce it. But the ground of our belief in the propagation of a disease by a volatile contagion being, that it is produced by exposure to the sick, without any other possible means of accounting for the phenomena, and never without such exposure, we are in this case entirely destitute of it, and

must conclude that typhus fever is not propagated by a volatile contagion.

It is next proposed to show that the remote cause of typhus fever is the dense gas arising during the decomposition of vegetable matter, commonly called miasmata.

In support of this doctrine it will be shown that the typhus fever arises in those situations in which miasmata abound, and in which in all parts of the earth miasmatic fevers are produced; that in the time of its rise and fall, its greatest height and lowest declension, it corresponds with miasmatic fevers, and that the same kind of weather is most favourable to the increase of both; that typhus fever on board of ships, at sea or in port, arises in circumstances in which miasmata abound; that the symptoms of typhus fever are the very same with those observed in fevers known to be produced by miasmata; that typhus fever appears in connexion with miasmatic fevers; and that they are mutually convertible into one another.

It is proposed in the first place to show that the typhus fever arises in situations in which miasmata abound. The first evidence I shall offer is from Dr. Currie's account of the diseases of Liverpool. He tells us that the town of Liverpool having been harrassed by typhus fever, the magistrates called in the physicians, to give their opinion of the measures it was advisable to take to improve the health of the city. The physicians, he informs us, took the matter into consideration and presented a full report on the subject, in which they took a view of the *causes* of the uncommon sickness of the two years preceding that time, and of the measures necessary to prevent its recurrence. In speaking of this subject they complain of the want of cleanliness in the streets, and mention ground within the liberties of the city which was not drained. They state that for twenty years before there had been collections of standing water and of filth of every kind in a part of the town which they named, and that that part of the town had been infested with low fevers, especially in the autumnal months.

These low fevers, occurring in situations where miasmata must abound in hot weather, and in the very season, the autum-

nal, when they are known to abound in such places, and therefore so manifestly produced by miasmata, are called by Dr. Currie without hesitation or question *typhus fever.* This he does not only in the general declaration before mentioned, that typhus fever is the common fever of England, the epidemic of all the great towns, but he calls this particular fever, typhus fever. Thus he states in two several tables respecting the health of Liverpool, the number of cases of fever, which were carried into the Dispensary in the same month: in one table the word fever is used, in the other typhus fever; in both places the same number of cases is stated.

It is evident from this statement that the miasmatic fevers which arose amid these stagnant waters and filthy places, were called typhus fevers by Dr. Currie.

Dr. Currie mentions a case in which a regiment billetted in Liverpool, and paraded and mounted guard in a fort on the banks of the river. The general guard-room was a very dirty and confined place, and had under it a cellar, which had been full of water in the winter, but was at the time in which the fever appeared, half evaporated and sending forth offensive exhalations. In this state of things, the typhus or jail fever broke out in June and spread rapidly.

He mentions another typhus fever which broke out in a regiment in barracks at Gosport. This is a town on the West side of Portsmouth harbour in the South of England. The land around this harbour is low and damp, and intersected with creeks. The ague and fever frequently prevails there and sometimes dysentery in the autumnal season. This fever commenced in May, progressed through July, and, though great efforts were made to suppress it, it continued through August and September, and ceased in November.

Manchester in England is situated between two rivers. Some of the streets were not drained at the time alluded to in the statement to be made. Dr. Ferriar tells us in his Medical Histories that the fever generally prevalent in this town and neighbourhood, in which it is evident miasmata must have abounded whenever the temperature was sufficient to produce

them, was a mild typhus. He expressly says "During the late epidemic it was observed that the fever prevailed most in streets which were not drained, or in which dunghills were suffered to accumulate" &c.

We have then, in these statements made by men who believed firmly in the contagious nature of the typhus fever, the most unequivocal evidence that typhus fever arises in situations in which miasmata abound in warm weather.

The next thing proposed to be shown is, that in the time of its rise and fall, its greatest height and its lowest declension, typhus fever corresponds with the miasmatic fevers of other countries. The reverse of this is confidently asserted: it is said that typhus fever is a disease of cold weather particularly; and that it disappears in hot weather. There is however on this point, as well as on the whole subject of typhus fever, a strange inattention to facts.

The following table shows at a glance that typhus fever is not a disease particularly of cold weather; that like all miasmatic fevers in temperate climates, it increases as the autumn advances; is at the highest in that season of the year, and declines on the coming of cold weather, and that it is particularly severe in the hottest weather.

A TABLE, showing the Number of Fevers admitted on the Books of the Liverpool Dispensary, from the first of January 1780, to the last of December, 1796, inclusive; distinguishing the Numbers admitted every Year,* and each Month of every Year.

Year.	*Jan.*	*Feb.*	*March.*	*April.*	*May.*	*June.*	*July.*	*August.*	*Sept'r.*	*Oct'b'r.*	*Nov'r.*	*Dec'r.*
1780	150	125	179	173	168	183	191	150	129	186	150	133
1781	130	146	180	200	187	154	157	127	167	234	208	223
1782	268	265	231	292	148	159	120	140	143	182	150	158
1783	210	158	184	207	222	212	286	227	265	316	257	273
1784	170	194	245	247	232	225	270	230	266	247	369	297
1785	235	268	296	294	219	187	173	180	186	250	244	182
1786	191	166	216	244	202	155	159	188	169	211	167	197
1787	259	209	301	234	313	356	255	192	218	234	283	326
1788	236	174	213	255	253	245	271	311	258	341	315	295
1789	319	176	338	323	391	205	184	162	212	214	204	208
1790	176	248	337	294	281	247	343	270	310	340	355	269
1791	253	247	277	230	233	240	266	248	300	344	335	371
1792	359	361	269	278	261	237	236	223	211	330	212	174
1793	174	209	221	259	237	334	199	197	338	305	224	228
1794	157	230	383	280	337	305	291	245	303	290	258	326
1795	152	265	546	204	234	230	248	159	196	239	317	180
1796	197	161	266	242	288	176	203	182	254	329	153	247
	3683	3602	4682	4256	4206	3850	3852	3431	3925	4592	4201	4087

*The right-hand column, stating the whole number of cases in each year, is omitted as having no bearing on the subject.

From this table it appears that typhus fever is not peculiarly a disease of cold weather. Under the name of each month is stated the whole number of the cases of typhus which occurred in that month for each of seventeen years; and at the bottom, the whole that occurred in that month for the whole seventeen years. The smallest of the sums-total in the table, excepting one, is under February, and the next under January. The whole number for the three winter months exceeds the whole number in the three summer months by only a very few cases.

This fever, like our autumnal epidemics, is sometimes followed by a winter epidemic precisely in the same circumstances. But notwithstanding this, the smallest number of cases in a single month occurred nine times in December, January and February; and only six times in June, July and August; and in four of these six, the number in the winter months was but a very few cases above that in the summer. On the other hand, the highest number in the year occurred only once in December and once in February; and once also in June and once in July. These statements show in the most pointed manner that this disease is not peculiarly a disease of cold weather.

But this is not all: It is not only not peculiarly a disease of cold weather, but it is particularly a disease of hot weather, and like all miasmatic fevers in temperate climates, increases as the autumnal season advances, is at the greatest height in that season, and particularly severe or malignant in the hottest weather, and declines on the approach of cold weather.

All this is shown by the table. The lowest total-number in the table is under August; it increases from above 34 hundred in that month, to above 39 hundred in September and near 46 hundred in October, and declines in November to 42 hundred, and to near 41 hundred in December.

This agrees with the increase and decrease of our autumnal epidemics.

In some of the years the autumnal increase is small in comparison with others, but it is invariable. There is a difference in every year, and the uniformity of the increase, however inconsiderable in some years, shows the operation of an uniform princi-

ple; just as theappearance of a few bilious cases in the autumnal season here, is a minor degree of the operation of the same cause that sometimes produces an epidemic.

This doctrine does not rest however on this table alone. Trotter and Currie mention many instances of epidemic typhus commencing in warm weather, increasing through the autumn, and declining on the approach of cold weather. Currie says the dysentery appeared in July 1801 in Liverpool, and afterwards the typhus fever raged with great violence. In August, September and October, there were fifteen hundred deaths, nine hundred above the usual number. In September six hundred and ten died.

The physicians of Liverpool, in the report to the magistrates already mentioned, allege the necessity of draining and cleaning the streets, because in their damp and filthy state, low fevers infested those parts of the town *in the autumnal months especially.*

Dr. Ferriar states that the weather was wet and warm in the summer and autumn of 1794, and that it was even a burning summer; and the typhus fever was unusually prevalent at the time. A bilious colic had raged among all ranks of people before the fever prevailed. The fever was at the same time prevalent in many parts of England, and in the fleets of England and France.

Miasmata in these circumstances must have abounded: it is impossible that it should be otherwise.

Dr. Currie mentions an epidemic typhus in the town or city of Canterbury in England. This town is situated in a low wet place; the summer was excessively hot. The epidemic commenced about the last of July, it prevailed through August and September, and the last case occurred on the 21st of October. The fever was accompanied by dysentery, the cases of which were very numerous.

On the first of June 1794, an English fleet captured most of the ships of a French fleet. That year was excessively hot. The French ships were very sickly, the typhus fever prevailing on board. The crews were landed at Plymouth and Portsmouth. The situation of these two places is such that it is impossible mi-

asmata should not abound in warm weather. Portsmouth in the hot summer of 1764, Lind says, was very much harassed by a miasmatic fever of high grade. During the continuance of the French in this situation, in this hot summer, there was a vast mortality among them, as well as among the militia who guarded them. In one place above three hundred died in four months, that is, in July, August, September and October.

This body of evidence shows, in the strongest manner, that typhus fever prevails most in hot seasons, that it increases with the increasing heat, and declines on the coming of cold weather. There is however in some instances a winter epidemic following the autumnal, precisely as in our epidemics. That this is the effect of cold operating on persons affected previously by miasmata, is evident from the identity of the symptoms in the typhus of winter and our bilious fever of winter; as well as from the fact that these winter typhus fevers occur in cold weather after sickly autumnal seasons. We have not time to dwell longer on this subject, but it may be illustrated by a single case.

In the year 1794, the summer of which was very hot, the number of cases of typhus in August was 245,* in September 303, October 290, November 258: here then is a rise from August to September and a gradual decline in October and November. In December however, there was an increase to 326 cases on the coming of very cold weather. In the more settled weather of mid-winter the number of cases fell down to 152 in January; and in February, but particularly in March, there was a great increase. This winter and spring were colder than had been experienced in England for a long series of years, says Dr. Currie.† The number of cases in December was 326 and in March 546.

This is precisely what occurred in 1814--15 in this country and in Virginia. In January snow covered the ground two feet deep and lay a long time, a most unusual thing; and the consequence was a very fatal epidemic in the latter part of winter.

The next thing proposed is to show that the typhus fever,

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*See the table, p. 74. †Currie's Med. Reports, p. 234.

which appears on board of ships in port or at sea, arises in circumstances in which miasmata abound.

There can be no question of this with regard to the ships in port, inasmuch as these harbours are almost all in the mouths of small rivers in a flat country. With regard to ships at sea, we have seen, when discussing the question of the origin of autumnal fevers on board of ships, that they abound in filth chiefly of vegetable matter.* They carry a vast quantity of wood, of vegetable substances of various kinds for food, and we know that the air of vessels is often so impure that lighted candles appear dim and sometimes the light goes out. In these circumstances, we have seen that miasmatic fevers of every form make their appearance.† Miasmata then abound in these circumstances, and in these very circumstances typhus fever arises. With regard to the temperature of the atmosphere when the fever appears on board of ships at sea, Trotter mentions a great number of instances in which it arose in hot weather. It is to be observed also that the temperature of the interior of a ship is always higher than that of the atmosphere without. The crowd of men, the number of candles continually burning, the fires that are necessarily burning in the hottest weather, all tend to produce this state; and we are told that on board the Immortalite, when the thermometer in the shade stood at 72° "the medium heat between decks, when the watch below were in bed, was 84°"‡

In the hot summer of 1794,§ when the mercury in the thermometer in the latter part of June stood at 80° in the shade, the typhus fever was general in the English, French and Portuguese fleets in the English Channel; and in the summer of 1779 the same occurred on board the French and Spanish fleets off Plymouth.‖

The French ships, a number of which were taken by the English in 1794, Trotter says, were extraordinarily dirty, and

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* Transylvania Journal Vol. 1, p. 317. † Ibid. p. 362.

‡ Trotter's Med. Naut. vol. 3, p. 98.

§ Ibid vol. 1, p. 78, 79, 89 and 197 compared. ‖ Ibid. p. 205, note.

great numbers of the seamen perished.* The English suffered less, because extraordinary care was taken to clean and ventilate every ship.†

The report of one of the surgeons, Mr. Gillespie, for July, shows the influence of high temperature in increasing the disease. He says, "The approach of the solstice and the canicula, seemed to have considerable influence on the spreading of the complaint; as notwithstanding the precautions used many were attacked with it."‡

The correspondence between the effect of the high temperature of the summer of 1794 by land and by sea, is worthy of particular attention. The same high temperature which produced an epidemic typhus fever on board the English and French fleets at sea, also produced an increase and continuance of the mortality among the French prisoners after they landed,§ upon their guard composed of English militia,§ an unusual number of cases in Liverpool (see the table), and an epidemic typhus fever at Manchester:‖ and there can be no reasonable doubt that it was general in England in that hot summer, as was, Lind says, a similar fever which prevailed in the very hot summer of 1765 in Portsmouth.

Dr. Trotter mentions many instances of this fever breaking out in single ships in the summer and autumnal months.¶ Several of these ships are mentioned as having been particularly dirty and leaky.

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* Trotter's Med. Naut. Vol. 1, p. 76, 77.

† Ibid. Vol. 3, p. 80. Vol. 1, p. 84, 86, 94.

‡ Ibid. Vol. 3, p. 90. § Ibid. Vol. 1, p. 84.

‖ See Ferriar's Medical Histories, p. 231. The passage runs thus; "In the summer and autumn of 1794 the usual epidemic fever" (which he says in p. 81 is a mild typhus) "became very prevalent among the poor in some quarters of the town, particularly after a bilious colic had raged among all ranks of people. To the ordinary causes of fever were now added the influence of a burning summer, succeeded by very wet, but yet warm weather," &c. Compare this with the passage, quoted a few lines above, from Mr. Gillespie, respecting the effect of the increase of the temperature of the weather in increasing the disease.

¶ Trotter's Med. Nautica, vol. 1, p. 135, 136, 137, 155, 165, 167, 178; vol. 2, p. 26; vol. 3, p. 59, 215.

The typhus fever was sometimes epidemic in winter in the fleet as on land. This was the case in March and April 1800, when the weather was cold, tempestuous and rainy;* and likewise in the same months in 1801.† It appeared at the same season in a number of ships not in fleet.§ The symptoms were "a combination of the symptoms of typhus with pneumonia";‖ that is, the disease consisted of the symptoms of the typhus fever as it appeared in summer, with the addition of those pneumonic symptoms produced by cold. There is probably more of the winter epidemic at sea than on land, for this obvious reason: when the weather is bad at sea, there is a greater necessity for turning out to save the ship; whereas on land, almost every body at such times is housed. Sailors therefore must suffer more in winter than lands-men.

It is evident from these statements that typhus fever appears on board of ships at sea or in port. in circumstances in which miasmata abound, and at those times in which miasmatic fevers appear. Excepting the month of March, in which when the disease appeared, it is stated that the weather was cold, tempestuous and rainy, the greater part of the instances in which the fever appeared, occurred in the summer and autumn.

It is now to be shown that the symptoms of typhus fever, as stated by the British writers, are those which we every year see produced by miasmata.

These symptoms, as stated by a considerable number of surgeons in the army and navy of England to Dr. Currie, but chiefly to Dr. Trotter, are the following.

The disease frequently commences with a dull headach with restlessness and shivering, pains in the back and all over the body, foul tongue, and great prostration of strength. The headach gradually becomes more acute, the temperature of the body increasing gradually till it attains a very considerable height, and frequently there is delirium in the fever. The eyes are sometimes blood-shot.

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*Trotter's Med. Naut. vol. 3, p. 160, to 174, 221. †Ibid. p. 194 to 209.

§Ibid. vol. 1, p. 57 to 67, 210; vol. 2, p. 14, 17, 21, 147; vol. 3, p. 60, 63, 153, 155. ‖Ibid. passim.

The pulse varies from a very weak and rapid, to a full, strong and hard stroke. This, strange as it may seem to those who consider this as a peculiar disease in which the pulse is always weak, is nevertheless a fact. The frequent occurrence of cases with hard and strong pulse in the midst of those with weak pulse, shows that the same cause produces both; and this connected with the identity of the symptoms in other respects, manifestly indicates that they are one disease. Indeed the question *of difference* seemed not to have arisen in the minds of these surgeons, and they therefore, speaking of it as typhus fever, state unequivocally that the pulse was sometimes hard and apparently oppressed.

With the headach and other affections of the head which often accompany pain in that part, there is nausea and vomiting, great anxiety, foul tongue, and parched tongue.

The discharges from the stomach and bowels are frequently bilious; those from the stomach sometimes green, and those from the bowels green, dark, sometimes black, and often very offensive.*

The skin in very hot summers becomes yellow. This is noticed by several surgeons, and in several years. One of the surgeons in his alarm apprehended that the yellow fever had been introduced, the ship having spoken a vessel from the West Indies and perhaps sent a boat on board. Trotter however relieved his apprehensions, telling him that it frequently occurred; and that he had often seen the eyes, the skin, and the urine as deeply tinged by bile in typhus, as he ever saw it in the yellow fever in the West Indies. He observes also that this symptom was particularly remarked in the warm summer of 1794.†

Trotter mentions a variety of symptoms occurring in the last stage of a mortal attack of typhus fever, of which I have seen all but one or two unimportant ones in our autumnal fevers, when patients were not properly evacuated; and those that I have not seen, others have; so that every symptom occurs in

*Hamilton on Purgatives; Darwin's Zoonomia, vol. 2, p. 490; Ferriar's Med. Hist. p. 71.

†Trotters Med. Naut. vol. 1, p. 259; vol. 3, p. 215, 218.

our fevers. Among these are mentioned in connexion hæmorrhages, intolerable stench of the breath and excretions; the skin dusky or yellow, petechiæ, vibices, *vomiting of black matter*, the stomach rejecting every thing immediately as swallowed,* &c.

This is a perfect picture of the fatal cases of our autumnal fevers, particularly when the patient has not been well evacuated; when from the violence of the case, or from the timidity of the practitioner, the patient lies without a sufficient discharge from his bowels.

Typhus fever has also its winter epidemics,† in which the symptoms are those which have already been mentioned, with the addition of such as are produced by the action of cold on the thoracic viscera, viz. pneumonic symptoms. This is what we see every year occur in our miasmatic diseases: they are likewise frequently followed by winter epidemics, in which the symptoms are those observed in the miasmatic fevers of the autumnal season, which are the very same observed in typhus, as has just been shown, together with the same additional symptoms in the thorax proceeding from cold, also observed in winter typhus.‡

It is next proposed to show that typhus fever appears in company with miasmatic diseases, and that they are mutually convertible into one another.

Dr. Ferriar, in his Medical Histories, tells us that he has seen cholera converted into typhus. He also states that dysentery and diarrhœa are often converted into the same fever. He mentions also an epidemic bilious colic, in the same season with an epidemic typhus fever.§

Pringle states that hospital fever, that is typhus,‖ and dysentery appeared together. Few escaped he says, for however mild or bad the flux was, for which the person was sent to the hopital, the fever almost surely supervened.¶ He states that an En-

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*Currie's Medical Reports, p. 25, 310, 388; Trotter's Med. Naut. vol. 1, p. 80, 109, 255, 266, 274. 281; vol. 2, p. 58; vol. 3, p. 55, 102, 161, 154, 209, 215, 218.

†See Transylvania Journal of Medicine, vol. 1, p. 537

‡Trotter's Med. Naut. vol. 3, p. 157, 191, 193; Ferriar's Med. Hist. p. 70.

§Ferriar's Med. Hist. p. 138. ‖See p. 62.

¶Pringle's Diseases of the Army, p. 25, 34.

glish army, which had been exposed in the autumnal season to miasmata in Holland, being on the voyage home in November, some of the men were taken ill with remitting fever, which was soon by the crowded state of the vessel converted into jail-fever,* that is, typhus.†

Ferriar states that in one case, in which a typhus fever was unusually protracted, the fever assumed the form of an intermittent.‡ Dr. Gomez, a physician to the Portuguese fleet, in a letter to Dr. Currie of Liverpool, mentions a case in which a typhus fever was converted into a dysentery.§

Trotter abounds with evidence of this kind. He speaks of a ship landing a number of men, after a voyage, ill with typhus and dysentery. In another case a considerable number of persons ill with flux and typhus fever, were received on board the hospital-ship from one of the fleet.‖ He speaks often of the typhus fever putting on the intermittent form, which, he says, showed it was on the decline;¶ and this very observation shows that the case was of so frequent occurrence that he had founded a rule upon it. He speaks of cases of typhus assuming a bilious remittent form.** He says that he has seen a family in which the father had an obstinate tertian, while the mother and part of the children had typhus fever. He states that he has constantly remarked, when typhus prevailed on board of ships, that intermittents and remittents occasionally appeared, and when the proportion of the latter increased, he considered it as a mark that the epidemic was on the decline.

The following quotations show this matter in a strong light. Speaking of one ship, he says "There were a larger number of bad cases in typhus, ague, and dysentery, than come usually to a naval hospital at one time. From the same transport and in the same regiment, were brought people ill of the three diseases just mentioned."††

"It is not easy (he says) to trace those circumstances, which

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*Pringle's Diseases of the Army, p. 39. †See p. 62.
‡Ferriar's Med. Hist. p. 82. §Currie's Med. Reports, p. 335.
‖Trotter's Med. Naut. vol. 1, p. 58; vol. 2, p. 14. ¶Ibid. vol. 1, p. 66, 70, 185.
**Ibid. p. 83, 85, 187, 188. ††Ibid. vol. 1, p. 192.

occasionally convert an intermittent or remittent fever into a continued type, (or typhus,) and vice versa.* There must however be something in their disposition very much alike; for wherever we find typhus affecting a number of people at a time, we also find cases of the remittent and intermittent form."†

These statements of facts, by men all of whom believed typhus fever to be propagated by a specific contagion, are abundantly sufficient to show that the fever called typhus and miasmatic fevers, including all the forms, intermittents, cholera, colic, diarrhœa and dysentery, arise together under the operation of the same circumstances, and are convertible into one another.

It appears evident from these statements that typhus fever bears the same relation to intermittents, cholera, colic, diarrhœa, and dysentery, that our remittent fever bears to the same.

Even the chronic diseases in which typhus terminates, are the same with those in which our autumnal fever sometimes terminates. Ferriar says "In particular seasons dropsy succeeds most cases of typhus," and mentions that this was unusually common in 1792.‡

It has now been shown that typhus fever arises in situations in which miasmata must abound in hot weather; that in the time of the rise and decline of this fever it corresponds with miasmatic fevers of temperate climates, and that the same kind of weather is favourable to the increase of both; that the symptoms of typhus fever are the same with those of autumnal fever; that typhus fever appears in company with miasmatic diseases; and that they are mutually convertible into one another. Whence it is evident that typhus fever is produced by miasmata.

The typhus fever though commonly called a continued fever is in reality a remittent. Cullen even admits it. He doubts

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*The single consideration, that intermittents are convertible into typhus, strongly indicates, indeed clearly shows the miasmatic origin of the latter. If the fever be first an intermittent and then a typhus fever, its origin is not changed; the typhus fever originated in miasmata.

†Trotter's Med. Naut. vol. 1, p. 311.

‡Ferriar's Med. Hist. p. 152, 232.

whether in forty years he ever saw a continued fever properly speaking. He says that there are almost always daily exacerbations and remissions in the most continued fever;* wherefore he makes remissions and exacerbations a part of his definition of continual fevers.

This being the case, it is apparent that the typhus fever of England bears the same relation to the intermittents, dysenteries &c. which, according to the declaration of Trotter &c. arise in company with it under the operation of the same circumstances, that the remittent fever of more southern regions bears to the intermittents, dysenteries, &c. arising in company with it, under the operation of the very same circumstances: and as the intermittents, dysenteries, &c. of England, are the same diseases with the intermittents, dysenteries, &c. of those southern regions, differing only in degree of violence, it is evident that the remittent of England viz. typhus fever, is the same disease with the remittent fever of those regions, differing only in degree.

Typhus fever therefore is the common autumnal fever, or remittent bilious fever of England.

This conclusion is supported by a variety of considerations.

1. The argument of Cullen, that remittents and intermittents are the same disease, because they arise from the same origin, prevail together in the same places, and at the same time of the year, and are convertible into one another;† is equally strong in favour of the identity of typhus fever and intermittents, these also arising from the same origin, prevailing together in the same places and times, and being convertible into one another.

2. Epidemic fevers are classed by Cullen under the head of typhus fever, which, from the situation in which they appeared, and the names given them by authors as descriptive of them, evidently were miasmatic diseases. Such are the epidemic diseases

*Synopsis Nosologiæ, &c. Tom. ii, p. 66, Nota. "Fere semper in febribus maxime continuis, exacerbationes et remissiones, etiam quotidie, satis manifestas observavi."

†Cullen's Nosology, Vol. 2, p. 56.

of Egypt which prevail most in the hottest weather and never after the marshes and lakes are dried up; the epidemic fevers likewise of Senegal, Hungary, Trent, Cologne, Warsaw, Leipsic, Cremona, Petersburg, Silesia, &c. all of which are so situated as to make it evident that they are liable to miasmatic diseases; some indeed are celebrated for them.

3. Cullen mentions under the head of typhus fever, the putrid fever of Macbride, the putrid catarrhal fever of 1768, the putrid bilious fever of 1769, and the putrid nervous fever of 1770, described by De Mertens.

These terms, putrid and bilious, were formerly used to designate the autumnal fevers of low countries.*

4. Lind gives an account, from the journal of a surgeon who sailed up some of the rivers of Guinea, of a fever which prevailed in the month of June in a town in 12 degrees north latitude. "Their sickness could not be well characterized by any denomination commonly applied to fevers; it however approached nearest to what is called a nervous fever (or typhus, see p. 64), as the pulse was always low, and the brain and nerves seemed principally affected."†

Cullen has also placed under the head of typhus, a fever called *Febris Senegallensis*, and refers to the London Medical Observations, II. 21, for an account of it. He therefore concurs with the surgeon above mentioned, in considering the fever of the West Coast of Africa, a typhus. Lind himself calls it a low malignant fever.‡ This fever however occurs in the rainy season, is of the remitting kind, and besides delirium, coma, sunk pulse, and a train of nervous symptoms, is accompanied by bilious vomiting and purging, and yellowness of the skin.†

It therefore originated amidst abundance of miasmata, and had the symptoms observed in fevers produced by that remote cause; and was therefore the ordinary miasmatic fever of that country.

Cullen himself has placed the same fever under the head of tertians also, viz. Febris remittens regionum calidarum.

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* See Pringle on the Diseases of the Army, p. 73, 168, 185.

† Lind on Hot Climates, p. 49. ‡ Ibid. p. 42.

5. In the year 1765 in Portsmouth in England, and in the neighbourhood, the summer being very hot, a violent fever prevailed, in which yellowness of the skin was a common symptom, with others which we observe in our autumnal fevers. This was called by Lind the autumnal fever of England.

In the year 1794, in the very same place, the summer of this year also being very hot, a very violent fever prevailed with the same symptoms, and among others, as occurred in 1765, there was yellowness of the skin. This fever Trotter calls typhus.* The identity of the two is palpable.

6. Another circumstance which shows the identity of typhus and miasmatic fever is, that physicians who come from England to this country, call our autumnal fevers typhus fever. Even those who formerly studied in Edinburgh, and their students, frequently do the same. Some years ago a physician from Ireland settled about a dozen miles from the place of my residence. In the following autumnal season he had a fever and requested my aid. He was advised to take a mercurial cathartic every day, with the assurance that he would soon be well. He was startled at the mention of such a course in his disease, which he pronounced typhus fever. It was the autumnal fever every where then prevalent.

7. The identity of *the cause* of autumnal fever and typhus is so obvious that Dr. Trotter is himself led to the conclusion, that these diseases have a common origin; but, what shows in a very strong light the propensity of the British physicians to believe in contagion, he concludes that dysentery and tertian fever are produced by the imaginary contagion of typhus fever. He says, "We have now seen in the Vengeance, the contagion of typhus, brought on board by two men, extend throughout a ship's company: and in the space of four months, in different subjects produce continued, remittent and intermittent forms of fever. In Lord Moira's transports, something of the same kind occurred, with the addition of dysentery. To these I might add other facts

* Trotter's Med. Naut. Vol. 1, p. 84.

in confirmation, from almost every ship which I have attended under a general contagion."

These facts numerous as can be desired, prove beyond question or reasonable doubt, a common origin; that they are produced by the same cause. What that cause is, cannot be doubted by those who see these fevers arise under the influence of what proceeds, in hot weather, from stagnant waters and piles of filth, or dung-hills as they are called, in the outer edges of cities, and disappear by filling up those places, or covering them with water, or by drying them up, or by draining them, or on the coming of cold weather. In short, the conclusion is inevitable that something *produced* in these circumstances is the cause; and that that agent is a gas, is equally certain when we consider the circumstances of the case at Gallipolis in Ohio, viz. that the fever appeared not until the wind blew over a neighbouring filthy pond upon the troops of the United States cantoned there, that it commenced with great violence immediately after the wind began to blow on them, and moderated and almost ceased on filling the pond with earth.*

Sir Gilbert Blane draws a similar erroneous conclusion. He observed the yellow fever arise on board of ships precisely in the same circumstances that give rise in northern regions to typhus fever. He therefore attributes them to the same cause, and ascribes them both to the contagion of typhus. He therefore calls the yellow fever *typhus icterodes;* that is, *typhus with yellowness of the skin.*

It is admitted that they are produced by the same cause. But we know that yellow fever arises in company with cases of common remittent fevers, with intermittents, with diarrhœa, with dysentery. We know that intermittents and dysenteries do not proceed from contagion of typhus, but can be produced with a plenty of water, in very hot weather, on any spot where vegetable matter is to be found; and when in full operation, destroying those subject to the action of the agent thus produced, the

*Transylvania Journal Vol. 1, p. 345.

whole can be controlled by carrying off or covering up the water. We know that they proceed from miasmata. Therefore also does yellow fever; and the specific contagion of typhus fever is unnecessary to account for any effect observed, and is therefore imaginary.

Blane endeavours to obviate this argument by alleging that the yellow fever produced by miasmata, and that on shipboard, are different diseases. But this attempt is a desperate one. For, it is evident that even on shipboard miasmata abound, not only from the quantity of vegetable matter on board and the high temperature of the holds of ships, but from the occurrence *there* of diseases known to be produced by miasmata on land.

Moreover the yellow fever produced on board of ships is not to be distinguished from that arising on land. The physicians from the West Indies who were in Philadelphia in 1793, and subsequently in other cities in other years, acknowledge the identity of the yellow fever of our cities with the yellow fever of the West Indies; and Blane himself destroys his own allegation by admitting that the yellow fever of Philadelphia of 1793 was the real yellow fever, alleging that the contagion was carried to Philadelphia from the West Indies.

This point is however settled by the occurrences on board of the two ships Hankey and Calypso at Bulama. There, on shipboard, arose the yellow fever, together with dysentery, intermittent fever &c. The latter we know, as has been stated frequently, arise from miasmata, because we can produce them at any time in hot weather with sufficient water, and remove them by carrying off the water in any way. Therefore as these diseases and yellow fever are evidently produced by the same cause, even to the satisfaction of both Trotter and Blane, at least on that point, the typhus icterodes is also produced by miasmata.

Notwithstanding the strength of the evidence on this subject, rendering it evidently impossible that the remote cause of typhus fever can be a specific contagion produced by the bodies of those who are suffering under this disease, there is a case of great notoriety which has been confidently represented

as settling the question of the contagious nature of this disease.

The case alluded to is that of the sickness which occurred during the sessions at the Old Bailey in London in 1750. A few years ago no one could argue this question without being told of this case, and it is still occasionally mentioned. It is however very remarkable that the bare facts of this affair show that, whatever else might have been the cause, it could not possibly have been contagion.

The court sat in a room crowded to the utmost by great numbers who were attracted by the desire of hearing the trial of an officer, who was charged with murder in consequence of having killed another in a duel. The trial continued all day. The house was very close, as it was cold weather though in May, a cold piercing wind blowing at the time. In a short time the air of the house became excessively offensive, as is very common on similar occasions.

In this state of things a number of persons, viz. the Lord Mayor, the judges and jury, and some other persons on the left hand of the Lord Mayor were, a few days after, taken ill, and a number died. Those on the right of the Lord Mayor escaped. The body of the people were not affected. This sickness was, and is even now by some, confidently ascribed to contagion introduced by prisoners awaiting their trial in the house.

To this it is objected, 1, that the prisoners are known not to have been sick; it is expressly so stated: 2dly, the persons affected were twenty-five feet from the prisoners, and those who were nearest to them were not attacked: 3rdly, it is known that even the contagion of small-pox cannot affect a person at that distance.

These are circumstances which are totally inconsistent with the nature of contagion; and if we look a little into some others, we may find little difficulty in explaining the manner in which the disease was produced.

It has been mentioned that the crowd rendered the air of the house very offensive. In consequence of this, a window was raised on the left side of the house. It was a wide one and the air was piercing. The judges on that side faced this window,

and the cold air blew immediately upon them. Therefore they, being next to the window and receiving the full impression of the cold air in a heated state, suffered; while those on the other side of the house, and the people below, over whose heads the wind blew, escaped. This explanation of the cause is given by Bancroft and is perfectly rational. But whether it be received or not, although such effects would at any time almost inevitably follow such exposure to cold in so heated a state as these men were in, it cannot be believed that contagion from people who were not sick, produced disease, not in those close to them and almost in contact, but in those who were at twice the distance which would secure them from the contagion of small-pox.

It is lastly proposed to make some observations respecting the treatment of typhus fever. Little or nothing can be added to what has been already said on this subject in the essay on autumnal fevers in the first volume of this journal.* The following case will perhaps be considered interesting, on account of the quantity of medicine required to move the bowels. The symptoms were those of what has been called the typhoid type of fever, and manifestly indicated that the patient was fast progressing into full formed typhus; and there is every reason to believe that if the disease had not been arrested, he would have died with the most aggravated symptoms of typhus fever.

During the present winter a member of the medical class had an attack of fever, and after a fortnight's continuance under the care of an able physician, was apparently recovering. He however suffered a relapse; and in consequence of some other engagement of his physician I was desired to visit him.

He had fever, with something of a hurried manner of speaking; but nothing very remarkable in the symptoms. He was surprised at the proposition to purge him, but submitted. He took several cathartics of aloes, calomel and rhubarb daily, without much effect. He was after several days observed

* Transylvania Journal of Medicine &c. Vol. 1, p. 377 to 384.

to be somewhat delirious, his tongue acquired a black colour, and he had some subsultus tendinum.

In this situation, having had no passage from three very large doses of the medicines above mentioned, his room-mate was desired to give him a drachm of calomel and a drachm of scammony at once. He did it with great apprehension, but found him manifestly benefitted by the operation that followed. He took for a week or ten days following, from half a drachm to a drachm of each of those medicines daily, and recovered perfectly, with an extraordinary appetite, without one particle of any other medicine.

I shall close this subject with expressing entire conviction that more patients will recover by the use of cathartics alone, provided they be so managed as to operate in the way proposed, viz. so as to produce consistent passages, than by any other mode of treatment whatever; and that the most unsuccessful practice is that which excludes cathartics altogether, relying on tonics and stimulants.

On the subject of regimen nothing has been said in the essay on autumnal fever; and therefore a few observations here will not be improper.

Sick persons should unquestionably be kept in a well ventilated room, but a current of air should not be allowed to pass over them. This may be effected without difficulty, by suspending a curtain in such a manner as to turn aside the air flowing into the room through a door or window. It may be done very conveniently, even when there is a strong current of cold and damp air, by letting the window curtain hang down in such a manner as to direct the current to the floor, entirely below the level of the patient in bed. In this way, even in winter, the cold air may be let in to ventilate the room, while the temperature is kept up by a fire.

The air of the room should be of a moderate temperature, such as is agreeable to people in health. The patient ought not to be covered with more bed-clothes than is agreeable to him.

The patient should drink only when he desires it, and his

drink should be of the temperature most agreeable to him. Cold water is in general most desired. Some object to the use of it while taking calomel. I will only state on this subject the result of my observation. In twenty-five years I have never debarred a patient from the use of cold water and ice in fever, notwithstanding the use of calomel; and have never seen one instance of evil arise from it. I have had patients who took a drachm of calomel at once, and ate ice all day after it, (and this more than once,) who were not salivated at all, and who suffered no perceivable inconvenience from it, but recovered a perfectly good state of health in a short time. My family has consisted of fifteen or twenty persons, almost every one of whom has in the same season taken calomel, and used cold water at pleasure without ever asking a question about it, and without any known evil consequence.

The patient should never be urged to eat. His appetite being gone and digestion impossible, to eat is to introduce into his stomach a mass to ferment and incommode him, sometimes to kill him. If he take any thing in fever, it should be well made chicken-water, or a similar fluid made with beef &c. but absolutely without any flour to thicken it. When the patient is kept from eating and purged in the way above mentioned, day by day, without interval, he will have almost invariably within twenty four hours of the time when his passages become natural in colour and smell, a good appetite. He ought then to have a nourishing diet, mild that it may not excite him, and fluid that it may pass easily along and not oppress him; such as mush and milk, soup with a little mutton or any tender meat well boiled.

ART. V.—*Miscellaneous Cases.* By JAMES CONQUEST CROSS M. D., of Courtland, Alabama.

THE celebrated remark of the great Rush, that no physician, however high he may stand in the scale of professional attainment, or enlarged and liberal may be his acquaintance with the collateral sciences, can ever reach an exalted degree of practical eminence, if he be not in the constant habit of keeping a faithful register of the singular and important cases that fall within the field of his observation, has by the universal consent of the ablest physicians become a *truism*. His memory, however retentive, can rarely enable him to draw with accuracy upon experience, swallowed up perhaps in the constantly increasing duties of twenty years' laborious practice. So changeable and chequered are the scenes through which he has to pass; so multiform are the circumstances which perplex and distract his mind; so endlessly multiplied are the varieties of abnormal action he is called to contemplate, and so jarring and conflicting are the effects his therapeutic agents produce, that, unless his endowments be extraordinary, and his faculties amenable to the most rigid discipline, at the distance of even five years, the ideas furnished him by his experience will be vague, inconclusive and confused; no clear, distinct and precise impression will remain upon his mind.

If, therefore, it be important to register our experience, it must be useful, to an infinitely greater extent, to disseminate that experience, should it tend to enlighten and expand the medical mind; to illustrate, bind and confirm doubtful practical precepts; to suggest and establish new and previously unknown principles; to spread new lights into the dark, trackless and unexplored solitudes of the science generally. The widely pervading utility of the practice recently come into vogue, of publishing *Hospital Reports*, though once violently contravened and adopted with great hesitancy in England is generally admitted,

and is at present, throughout the greater part of literary Europe, the prevailing custom. Nor has experience of its merits tended in the least to undervalue the original prospects of utility it so alluringly held out; for in England, where it had to encounter the most steady and obstinate resistance, it is now uniformly insisted upon, and the chief of the metropolitan physicians have come to the wise resolution, of having exposed to the keen edge of scrutiny, too often sharpened by anger and malevolence, their practice in the most important diseases that come under their notice.

Cases treated in public institutions certainly deserve more confidence than those that emanate from the sources of private practice. Their authenticity arises from the plain fact, that the accounts given of them are more apt to be true. The physicians of such establishments have but little opportunity to impose upon the public, as the hazard of detection is too imminent; they are operated upon by too many restraining circumstances, to venture to impart attributes to a disease which it never possessed, or to give credit to a therapeutic agent which it obviously does not deserve.

'T is true, the public has a right to be sceptical, to a certain extent, with regard to the histories of cases that have occurred in the sphere of private observation; for all the fascinating allurements of novelty, of exaggeration, and of perversion, and the perfect security from detection, rush in all their force upon the physician, without a single antagonizing agent, save his frequently too pliant conscience, to deter him from wilful misrepresentation. These characteristic circumstances, therefore, clothe in a certain degree of doubt all those cases treated in the wards of private families; while those issuing from hospitals come to us pure and unadulterate from the hands of nature. This, indeed, is a great, but it is the only superiority which cases occurring in public have over those that take place in private practice; while the latter, when faithfully narrated, frequently offer a degree of interest never possessed by the former. The patient who is pampered by all the refined elegancies of affluence, surrounded by officious, garrulous and gossipping acquaintances,

and exposed to the dangerous and superserviceable zeal of tender relatives, frequently presents developements rarely, if ever, witnessed in the hardy weather-beaten tenant of a hovel, when subjected to the rigid regulations of a hospital. When, therefore, the physician suffers himself to be the author of intentional misrepresentation in any shape, he becomes unmindful of his own real interest; he forgets what he owes to humanity; how closely he is connected with the dearest concerns of his profession; and by bringing into disrepute a fountain so rich in valuable knowledge, and abundant in the materials of a noble science, he must unavoidably, if the sensibilities of his soul be not already stagnant, arouse the indignation of a troubled and violated conscience. It is a mine overflowing with a superabundance of the most valuable ore, but the coin manufactured from it is considered worthless and counterfeit, because it has been so frequently alloyed by admixture with the more dazzling but baser metals.

The extreme scarcity of public institutions for the reception of the diseased, both in the West and South, from which cases bearing the stamp of authenticity can emanate, together with the universally acknowledged importance of genuine histories of disease, should prompt the physician to the best exertions of an honest heart and a sound head; that the only source from which he is permitted to derive practical knowledge may be redeemed from a too well merited disgrace.

Case 1*st. Ovarian Dropsy.*—At the very threshold I am reduced to the necessity of experiencing all those disadvantages to which I alluded, when speaking of the propriety of keeping a Case Book; for although I noted down at the time the character of the disease, and gave a detailed epistolary account of it to my distinguished friend and preceptor, Professor Richardson, soon after it came to my knowledge, my notes have been lost or mislaid, and consequently I am precluded from giving as exact an account of it as it deserves. Two striking features, however, are fresh in my recollection, which will I hope be considered ample apology for my intruding it upon the public.

In the year 1822, while practising my profession in Gallatin County, Kentucky, I was summoned to treat Mrs. G. for bilious fever. When I arrived I found her quite sick and labouring under that disease, then prevalent. Before I prescribed I was informed by her husband that she had been troubled, during the space of four years, with a tumour of considerable size, situated low down in the abdomen, I think in the right side, and that she had been under the care of a number of medical gentlemen from whom she was unable to derive the smallest benefit. He appeared anxious that I should undertake her case, which I agreed to; but determined to postpone all exertion to remove the tumour till such time as she should be freed of her bilious affection, and restored as far as possible, in other respects, to good health. Upon examination I found a swelling low down in the abdomen, inclined deeply to one side lodging in the hollow of the ileum. It was about the size of a child's head at birth; it was rough and tuberculated; it distinctly fluctuated and pulsated like an aneurismal swelling. The disease made its appearance nearly four years before, and although it continued to increase in magnitude, she did not appear to have suffered much in her general health. During this period she had conceived once, and had given birth to a healthy vigorous child, that was at the time I saw her, about eighteen months old.

Treatment for bilious fever was immediately instituted, and by the energetic employment of the lancet, of mercurial and aloetic cathartics, the disease gave way in the course of three or four days. On the last day that purgation was deemed necessary, an infusion of Senna was given to accelerate the operation of the medicines just mentioned. When I visited her the succeeding morning, I found her nearly, if not quite well, and was surprised to learn from her, that the tumour had disappeared. An examination was immediately instituted, both externally and per vias naturales, and to my unspeakable astonishment, (for I was incredulous before,) I found her statement literally true. There was no vestige of tumour to be found. Upon inquiry as to what had transpired during the previous night, I was informed, that she had had several copious watery evacu-

ations from the bowels, and that she had urinated to an amount that not only astonished but alarmed her. Considerable exhaustion was the result of this excessive action of the bowels and kidnies, but it was transient, as her natural powers soon relieved her.—The tumour did not return during the four or five subsequent months that I remained in her vicinity, and since I have been unable to learn whether or not the cure was radical.

Remarks. The attributes of this case which I consider particularly interesting, are the pulsation of the tumour, and the extraordinary action of the Senna. In all the references I have been able to make, I have not succeeded in finding a parallel case on record; in no single instance, has the phenomenon of pulsation made its appearance. The most rational explanation that occurs to my mind is, that this characteristic must have been imparted to it by the external iliac artery; yet, to this there are two formidable objections. The ovarian swelling has generally the same location, and if the normal vigour of the heart could produce pulsation in a dropsical tumour in one, it should do so in every instance. But as this has never before occurred, so far as my acquaintance with medical writings extends, the presumption may naturally arise, that in this case the heart may have, from some cause, acquired a certain degree of morbid energy. No discovery of this kind, however, could be made; the circulatory apparatus appeared to be in good health, and moreover, if a cause of this kind could have had any agency, it must have existed during the whole period of the disease, a time generally sufficient for an affection of the heart to extinguish life; for the pulsation was obvious to the patient from the time she made the discovery of the swelling.

It is a conspicuous feature in the character of ovarian dropsy, to be particularly rebellious under the use of internal remedies; we would, therefore, call the attention of the profession to the anti-hydropic power which the Senna exercised in this case, as it may possibly possess some peculiar virtue, that particularly adapts it to the removal of the species of dropsy under consideration. Since the occurrence of the case we have above recorded, we have not had a second opportunity of ascertain-

ing more fully, the extent of its efficacy; and while we would suggest a trial of it, we would disclaim all idea of recommending it as a sure or certain remedy.

Case 2d. Ascites.—In July 1827, I visited for the first time Amanda, the daughter of Miss B. of this town. She was nearly six years of age; of good consitution; sanguine temperament; had enjoyed good general health till the previous autumn, when she had an attack of intermittent fever, which was protracted and left an enlarged and indurated spleen. The succeeding spring her abdomen began to enlarge, fluctuation could be sometimes detected, and sometimes not. Other symptoms were also present which induced her mother to employ medical aid: the treatment pursued, however, I am unable to notice as I had no acquaintance with the case at that time. Several physicians had given her medicine before I saw her, but without any alleviation of her sufferings. When I saw her she had an enormous abdomen: fluctuation was very manifest; inextinguishable thirst; slow bowels; foul tongue; quick, rapid pulse; occasional wandering pains in the abdomen which were not increased by pressure; feverish skin through the day, and always high cutaneous excitement late in the evening, and early in the night; insatiable appetite.

This case I considered, in its pathological character, *Acute Symptomatic Ascites*, associated probably with *tinea*. During the first week of my attendance she was bled repeatedly, probably to the amount of ℥xxiv: she was purged with cremor tartar, jalap, sulphate of magnesia, and once or twice with calomel; she took diuretics of scilla maritima and digitalis purpurea; and, under the impression that tinea existed, the oil of turpentine in considerable doses was given. About the first of August she appeared to have derived great benefit from the course that had been pursued: her abdomen was much reduced in size, and the symptoms generally had diminished in intensity. The digitalis now, however, from some cause unknown to me, ceased to act upon the kidneys; this reduced me to the necessity of substituting some other diuretic. Until the 22d of August

I kept her under the operation of pills composed of sub-muriate of mercury, scilla maritima and pulvis antimonialis. Each pill contained five grains of mercury, three of squills, and one of antimonial powder; and they were exhibited three times a-day. During most of the time she took these medicines, the characteristic mercurial effects were developed; the gums, tongue &c. being sore, and the saliva flowing in great exuberance from the mouth. Under their agency the abdomen was reduced nearly to its normal size; the pulse, though a little quick, became regular; the skin natural in temperature and function; and so general an amelioration took place in the other symptoms, that I flattered myself with the expectation that my patient would very soon realize an entire restoration of health. Thinking it injudicious to push the mercury farther, from a persuasion that additional benefit could not be derived, we suspended it, and continued the employment of the other articles for the space of ten days, without, however, obvious benefit continuing to result; the disease remaining to all appearance *in statu quo.* Her strength being a good deal reduced, and the fever occurring rather periodically, to restore the one, and arrest the other, the sulphate of quinine was given in grain doses three times a-day, for three days, when the sub-muriate, in conjunction with the other elements of the prescription, was again resumed. The salivation by this time was checked, and the mercurial ulcers cured. The plan just alluded to was pursued for three weeks, during which time the hydropic symptoms daily gave way, and at the expiration of it we did not believe there existed a drop of dropsical serum in the abdomen. Salivation was again induced; the mercury was suspended and nothing but a solution of the sulphate of iron was given to act as a tonic. In the course of a few days shooting pains in the abdomen were developed, together with constant but ineffectual efforts to vomit. The iron was suspended, and a blister applied to the epigastrium which tranquilized the stomach. There was also great intestinal torpor, which required the most energetic drastics to keep up sufficient purgation. The abdomen continued of its healthy magnitude till the 22d of September, when, in

the course of a single night, it transcended considerably in size what it had ever been previously. The swelling now invaded the chest, neck, face and particularly the eye-lids, and subsided again, without any obvious cause, without any sensible excretion, either urinal, fecal or perspirable, during the night of the 25th. Great difficulty of breathing, constant flatulence *sursum ac deorsum;* bowels torpid, and alvine evacuations when induced by cathartics attended with great pain, remained after the subsidence of the abdomen. Pills of calomel, squills and pulvis antimonialis, in augmented doses, were again resumed. Cremor tartar and jalap were given to act upon the bowels. This plan was persevered in off and on, (for I found it the only course of any benefit) till the last of January 1828. During this period, she was several times salivated, and as often freed of the dropsical effusion; but whenever it was suspended, as was frequently necessary from the debilitated state of the system, it returned with astonishing punctuality and despatch. I essayed the scilla maritima and pulvis antimonialis alone, but with no encouraging result; nor did calomel by itself seem to possess near the active power in removing the serum, which it did when conjoined with the articles just enumerated. Early in February I was again flattered, by the fallacious expectation, that I should ultimately accomplish a radical removal of the disease. At that time, she laboured under profuse salivation; the abdominal tumour entirely subsided; all fever disappeared; the pulse became slower and more regular; the intestinal tube expelled its contents freely, sufficiently often and without pain. By the last of February, however, the water had again accumulated, but not to so great an extent as at several previous periods; and apprehending danger from excessive ptyalism, I determined to suspend the sub-muriate and to substitute in its place the tincture of iodine; knowing it from the statement of others, and the experience of myself, to possess peculiar powers in exciting the vessels of the absorbent system. It was given at first in doses of eight drops, three times a day, each dose to be increased one drop every third day, until the 10th of March. During this period the bowels became torpid; the ca-

thartic powers of the colchicum, together with the eulogistic praise bestowed upon it by Scudamore, especially when associated with the sub-carbonate of magnesia in dropsical affections, recommended it to my notice. The wine of colchicum and the sub-carbonate of magnesia, of each a tea-spoonful was therefore given three times a-day, and continued without interruption until the 15th of April. No benefit, however, was derived; the disease continued progressively but slowly to increase, until it resumed its former enormous magnitude. On the 3d of May her abdomen was very large, and she was examined by Doctors Gray, Mosely and myself. To my astonishment and that of Dr. Mosely, after the most attentive examination in every possible attitude, no fluctuation could be detected. This phenomenon when there existed abdominal distension, was never absent on any former occasion; indeed it had always been clear and distinct. Dr. Gray however, differed with us in opinion; he was persuaded of the existence of fluctuation, but in so slight a degree as barely to be perceptible. At this time I left Alabama for Kentucky, when she was put under the exclusive care of Dr. Gray. Medicines of various kinds were given by him but ineffectually, and considering that every therapeutic agent had been fully tried that promised benefit, he determined, as a last resort, to tap her. The subjoined extract is from his notes of the case. "Supposed to be dropsical; abdomen much distended; general health tolerably good. May 25th. Tapped this morning about one inch below the umbilicus; no discharge of either water or wind. Immediately after withdrawing the lancet, there followed about a tea-spoonful of something resembling *cheese-curd*. The canula was introduced but there escaped neither fluid nor wind. The orifice was closed by adhesive strips, and cremor tartar and gamboge ordered to keep her bowels soluble." The orifice closed kindly by the first intention. Without any obvious cause, though there had taken place no discharge of serum or air from the wound at the time, or after the operation, the abdomen subsided and the swelling never returned. Her general health did not decline, nor did any premonitory symptom indicate approaching dissolution, yet she ex-

pired about four weeks after the perforation of the abdomen. The prejudices of the mother and friends were insuperable; consequently no autopsic examination could be made.

Remarks.—The unfortunate and unavoidable circumstance of not being able to examine the abdominal viscera after death, renders the history of this case necessarily imperfect; we are still left in doubt with regard to its real nature, and this at one time induced me to decline publishing it; but reflecting more maturely on the subject, I came to the conclusion that defective as it is it will still constitute an important item in a detailed history of dropsical affections. While lamenting this circumstance, permit me to remark, that I may not be accused of neglecting the study of pathological anatomy, that in this country the prejudices against post mortem examinations are peculiar and invincible, as is evinced by the fact, that I have been permitted to open but a single patient since I have been in the state.

This case, though not so interesting in a pathological point of view, as it would have been had a dissection been allowed, presents some peculiarities of a therapeutic character, that should not be overlooked. It beautifully illustrates the propriety and excellence of the practice of exhibiting mercury in large doses, in acute dropsical affections, as recommended in my "*Essay on Dropsy.*"* We discover that whenever the mineral was enabled to produce its characteristic effect upon the system, that the dropsical affection soon disappeared, and that whenever its use was suspended, it rapidly returned. This was invariably the case, though the system was several times under the active operation of the most powerful remedies, clearly evincing the complete dominion which mercury can exercise over the secretion of dropsical serum. I would also direct the attention of the reader to the subject of mercurial salivation in children, a point particularly noticed in the essay already alluded to. During the treatment of this case under consideration, profuse salivation was produced no less than six different times, and instead of endangering the life of my patient, or of injuring her in any

*Medical Recorder, Nos. 39, 40.

other respect, than the production of moderate debility, the more decisive and promptly salutary effects were produced. The debility, when it did occur, was readily removed in the course of a few days by the temperate use of the sulphate of quinine, and sometimes by the sulphate of iron, which I employed upon the suggestion of a professional friend, and have found it admirably suited to the purpose for which it was given.—When salivation takes place it should constitute no reasonable ground for serious alarm; it may be readily restrained within innoxious limits or be in a very short time entirely checked. The entire truth of this remark is not only verified by the case just narrated, but by a number of others that have fallen under my notice. The treatment, however, which I have been in the habit of pursuing in mercurial salivation, ulceration and gangrene, is not generally known to the profession; it has proved in my hands and those of others uniformly successful. Indeed, I have never lost a patient from the excessive action of mercury, and should my success continue, I shall, perhaps, at a more suitable season, and when my experience shall have become more diversified, be induced to throw it more immediately before the public.

Case 3d. Cephalalgia.—Visited Mr. R. W. last February; aged 64; naturally lean habit of body and much emaciated by protracted suffering; sanguine temperament; rather irregular and intemperate liver. During the three months preceding my first visit he had laboured under the most excruciating pain of the head, throughout the whole of its occipital portion. The pain was so constant and exquisite, that he was confined at least four fifths of his time to bed; nor could he when he desired it raise his head from his pillow without assistance; and even then it extorted from him cries and groans so piercing, (though a man when in health of considerable fortitude,) that they would have awakened to sympathy the most insensible and obdurate bosom. He was reduced to a mere skeleton, so long and so intensely had his disease preyed upon him; nor did his mind in the gradual mouldering of his frame, escape unhurt. He would frequently

in moments of anguish quite indescribable, become perfectly frantic, and even in calmer moods, his mind would wander wildly and unintelligibly upon subjects for which he betrayed no particular predilection when most clear and unembarrassed. He was harassed by an almost uninterrupted insomnium, and when he could procure a few moments of repose, it was unrefreshing and disturbed by terrific dreams. His pulse was feeble, quick, and frequent, amounting to 120 beats in the minute; skin a little feverish; tongue covered with a thick, dark, furry coat from the root to the apex; almost constant nausea; bowels generally torpid and always slow in acting when under the influence of cathartics; and appetite entirely extinguished.

He had been under the care of several physicians from whom it did not appear that he had derived any benefit, for he declared that his torture, when I first saw him, was as intense, if not more so, than it had been at any previous period. At his own request, I detracted ʒx. of blood, but it afforded no relief; and believing his disease to be exclusively chylopoietic in its character, I prescribed rhubarb, blue pill, calomel and a variety of other purgatives during the space of two weeks, together with some other unimportant auxiliaries, without, however, advancing a single step towards a cure. The sulphate of quinine, cantharides to the skin, warm pediluvia and maniluvia, (for his hands were generally cold,) were then tried; but all to no purpose. Although the cathartic medicines that had been given had acted well, having brought away very dark, bilious evacuations; though the morbid condition of the digestive apparatus appeared to be rapidly ameliorating, and his strength recruiting; not the slightest impression was made upon the pain of the head. Sleep was still impracticable, at least that which is sound and refreshing; I therefore, determined to procure it, if possible, without too much aggravating the pain. The common liquid preparations of opium invariably increased his suffering, without affording him repose; the black drop was then had recourse to, and repeated in pretty large doses till he fell asleep. He slept until midnight, and then waked and was restless; the medicine was again given which procured him

sound repose till day. The pain was not increased as had been uniformly the case when under the operation of common laudanum or opium; it however, returned a short time after he awoke with all its usual violence. The great desideratum of procuring rest, without increasing the pain subsequently, was now within my reach, and I felt gratified at having gained even this little ascendency over his disease.

Sufficient trial, I thought, having been made to convince me that acting by purgatives and alteratives upon the chylopoietic organs would not effect a cure, I resolved to employ the tincture of iodine. To this I was led, in a great measure, by having seen a case recorded, in which it had succeeded. I gave it in doses of ten drops three times a day, each dose to be increased one drop every other day. The black drop was continued every night. The relief obtained was manifest; in two days the pain was evidently more sufferable; the third day he could raise his head from the pillow without assistance; the fifth he could walk slowly about his room without increasing the pain; by the ninth day he complained of no pain, was cheerful and appeared the most grateful man I ever saw. After the twelfth day he abandoned the use of the tincture altogether. In the course of the third week he was walking about town, and has continued to enjoy uninterrupted good health ever since.

Case 4th. Phrenitis.—W. S. æt 26; rather lymphatic; subject annually to autumnal bilious affections, but enjoying in other seasons sound and vigorous health, had complained during the period of three weeks previous to his attack of slight cerebral disturbance. This he attributed to exposure to an ardent meridian sun for the space of four hours, when the thermometer stood at 91°. Constantly engaged during this time in active military exercise, he became excessively heated and fatigued, and drank a large quantity of cold water. I visited him for the first time the 17th of last July. Delirious, wild and frantic gesticulations, deeply flushed face, injected eyes, intolerance of light, insomnium complete, pulse full, hard and not more than

sixty. Bled him 3xxiv; cold applications to his head; ankles blistered to act revulsively; cathartics of calomel, jalap and aloes in heavy and frequently repeated doses. At 3 P. M. medicines had operated well, but without perceptible alteration. Bled him 3xx, and continued the cathartics.

18th. At 10 A. M.; pain still intense; still delirious but not so wild or constantly; other symptoms but little ameliorated. Bled him 3xvi; continued the cathartics.

19th. At 10 A. M.; pain still considerable, but little intellectual disturbance. Bled him 3x, and continued the cathartics in reduced doses.

20th. At 11 A. M.; pain intense and an increase of mental wandering. Bled him 3xvi; and enlarged the purgative doses. At 1 P. M. the disease appeared suddenly to cease; no cerebral pain; no delirium; talked coherently, and we anticipated a speedy recovery.

21st. At 8 A. M. the disease had returned but did not exhibit as much violence as it had on the morning of the preceding day. As the extensive depletion he had undergone had considerably enfeebled him, I determined to suspend every kind of medicine for the space of twenty-four hours. Pain and other symptoms subsided at 12 o'clock, and with the exception of physical debility, and slight vertigo, he appeared to be entirely free of disease.

22nd. At 12 o'clock. Had laboured during the morning under sharp pain, which suddenly disappeared about 11 A. M. The disease was now manifestly intermittent in its character, and I determined to treat it in future by tonic and stimulant medicines. Two grains of the sulphate of quinine, four of the sub-carbonate of ammonia, and eight of rhubarb were given every three hours.

23d. Visited him at 11 A. M. No pain; nor did it come on till 3 P. M. and then, though intense, it did not equal in severity that of the preceding day.

24th. Under the impression that an emetic would promote the action of the tonics, six grains of tartar emetic were exhibited which produced plentiful emesis. When the stomach became calm the articles of the preceding day in the same doses

and at the same intervals were administered. This day he was tranquil and perfectly free of pain.

25th. Same medicines continued which prevented the recurrence of pain. This treatment persevered in for a week, though in gradually reduced quantities, restored him to entire health.

Remarks. The chief feature of interest in this case, is its manifestly phrenitic character during the three first days, and its afterwards assuming the intermittent character of cephalalgia. This circumstance would induce me to believe, but for the intensely violent nature of the inflammatory symptoms, that it was simply a severe case of cephalgia. At the onset, however, it was marked by too many plain and distinctive characteristics, to permit mistake; all the conspicuous symptoms of encephalic inflammation were developed with great vehemence. Perhaps the "*eleves*" of the "*nouvelle doctrine*" may consider this case as presenting a pathological phenomenon illustrative of the existence of intermittent "*gastro enterite*," and though an "*entologist*," in the language of the illustrious author of the "*doctrine physiologique*," we scorn disguising any fact, that may contribute to sustain the views of the French school, or aid in eliciting truth.

Case 5th. Threatened Abortion.—On the 11th of last January, I was called to Mrs. S. of this county, who was supposed to be on the eve of miscarriage. She is about 27 years of age; large, muscular and masculine in appearance; sanguine temperament and belongs to the labouring class of people. She had been busily engaged in her domestic concerns during the day, and attributed the symptoms under which I found her labouring, to over-exertion. The fifth month of gestation had been accomplished according to her computation, and she had experienced symptoms which authors attribute to quickening. I saw her about 10 o'clock at night, and ascertained that she had been seized about six hours before. When I saw her she complained of occasional periodical pain in the back, which extended round towards the umbilicus, and descended in the di-

rection of the groins: these pains had harassed her, off and on, from the time of her seizure, but not always with the same intensity. They appeared however, upon the whole, to have progressively increased in violence. She also complained of considerable pain, and a sensation of heaviness in the hypogastric region. The skin and pulse were febrile, and there was some cerebral excitement. From the commencement there was a scanty discharge of clotted blood from the vagina, which had increased to a smart flooding. About half an hour before my arrival, I was informed by her mother that the liquor amnii had passed off. An examination was made per vaginam, when I found a considerable descent of the uterus in the pelvis, and its mouth expanded to about the size of a twenty-five cent piece, from which she was bleeding with considerable freedom.

Considering abortion inevitable, to promote dilatation of the os uteri, to accomplish delivery as soon as practicable, and to restrain hæmorrhage, I bled her ʒxii; administered a cooling clyster, and made cold applications to the hypogastric region and vulva. These things being done, I determined to await the natural efforts of the womb, without further interference unless some exigency should demand it. In about an hour the flooding subsided, and though the pains returned at very distant intervals, they gradually diminished in intensity, and towards morning entirely disappeared. Knowing the fœtus to be alive from the undiminished plumpness of the breasts, and its vigorous motion in utero, together with the total disappearance of all those symptoms that indicated approaching parturition, I conceived the possibility of enabling her to retain the ovum, until the natural expiration of gestation. She was directed to remain in bed for several days, with her hips elevated; to keep perfectly cool; to take an occasional laxative; to be bled if the pulse indicated it, and to take a tea-spoonful of laudanum, should there be a return of pain or hæmorrhage.

I left my patient in the morning, and did not hear from her again until the 30th, when I was again sent for, and found her nearly in the same situation. She had pursued quite strictly the advice I gave her, until a few days before I was a second

time summoned, when she thought herself so much improved as to justify her in returning to her domestic duties. Too much exercise however produced threatening symptoms. She had pain in the back and hypogastric region; slight expansion of the os uteri and hæmorrhage, but not so profusely as the first time. She was bled, a tea-spoonful of laudanum given, and an anodyne injection administered. The pain and hæmorrhage diminished, but did not entirely cease, and three hours afterwards the same dose of laudanum was repeated. At the expiration of two hours she appeared perfectly tranquil; the pain and hæmorrhage had both disappeared. Quietude, an occasional anodyne and cooling purgatives were ordered during the remainder of her pregnancy, the usual termination of which she succeeded in reaching, when she became the mother of a fine healthy child.

Remarks. From this case we are led to believe, that accoucheurs are generally too positive and unqualified in their rules of practice; that they would without a dissenting voice, have pronounced abortion inevitable in the above case, when the result clearly proves how egregiously they would have been mistaken. Upon practical points of such moment it is extremely important that practitioners of obstetrics should be, if possible, definitive and settled, as upon their conduct, when abortion is threatened, the life of a human being may depend. Moreover, rules that exclude all hope may induce the inexperienced to wait supinely for the result, when it is possible that exertion may succeed in doing good, when it can possibly do no harm. I do not mean that we can always patiently wait with impunity, for there are circumstances when such a conduct would be highly culpable; but cases do occur when abortion may appear inevitable, and yet no symptom present that should urge us to act precipitately. Thus, it is a uniform sentiment among obstetricians that a discharge of water from the womb, occasioned by a rupture of the membranes of the ovum, at any period of pregnancy, is necessarily followed in a short time by parturition. The error of this sweeping declaration is exhibited by the case just recorded. But it may be objected

that every watery discharge from the vagina is not a discharge of liquor amnii, and that it is more than probable the discharge, to which we alluded in the above case, proceeded from the glands about the neck of the womb. I was aware of this and inquired particularly as to the manner in which the fluid came away, and was informed that it was neither a continual oozing such as takes place from the relaxed glands about the neck of the womb, nor a dribbling away of the waters, such as sometimes arises from a small rupture of the membranes of the ovum, but a sudden gush of a considerable quantity of water accompanied by the severest bearing down pain she had experienced. She was unable to inform me whether there was a subsidence of the abdomen immediately afterwards or not, but I remarked a degree of flaccidity in the *parietes abdominis* not common at the fifth month of gestation.

It is remarked by Mr. Burns, that "when the action of gestation ceases universally in the uterus, another action, namely muscular contraction begins, and then all hope of retaining the ovum any longer is at an end." The following I presume are the symptoms which he supposes indicate the existence of "*muscular contraction.*" "Whenever then" he remarks, "at any period of pregnancy, *we have paroxysms of pain in the back and region of the uterus*, more especially if these be attended with a *feeling of weight in that region*, tenesmus, micturation, *descent of the uterus in the pelvis*, and *opening of the os uteri*, we may be sure that expulsion, though retarded will soon take place." Other authorities might be quoted, but they so strikingly coincide with Mr. Burns, that it would be simply a repetition of what he has said. Moreover, this is amply sufficient, as it exhibits fully the opinion of perhaps the ablest obstetrical author and practitioner living; and while we would sanction by our humble support, most of his practical opinions, we would venture the assertion that the precept, founded upon the above symptoms, has sacrificed its hecatomb of children.

(TO BE CONTINUED.)

ART. VI. *Observations on the Predisposing Cause of Fever.* By ELIJAH COONS M. D., of Moulton, Alabama.

No disease is more diversified than fever. Hence the discrepancy of opinion, in the medical world, respecting its ætiology. To enumerate the various opinions from the time of Hippocrates down to the present day would be a work of volumes. Suffice it to say, that we believe in the miasmatic origin of autumnal fever. In support of this opinion, notwithstanding much valuable matter has recently been given to the medical public on this subject,* we propose making a few remarks. Though I do not pretend to any thing entirely new, yet I hope that my remarks will answer as collateral evidence confirmatory of what has been stated.

In reviewing my notes on autumnal fever, as it appeared in this vicinity in 1827, I find the following, after some preliminary observations, assigned as the cause.

The predominant cause, I believe, was malaria or vegeto-animal effluvia. The subsequent reasons are offered. The fever prevailed in the neighbourhood of a large lake or pond, which, in the winter and spring of the year covers an extent of country between two or three miles in circumference. During the vernal season vegetation springs up luxuriantly from its bottom, the water being shallow; and in the summer its whole surface is nearly covered with a green pellicle or coat of vegetable matter. Towards autumn this pond nearly dries up, unless the summer be wet. The decay and putrefaction of these aquatic plants and grasses, and that of myriads of small animals rapidly ensue upon its exsiccation, which afford an abundant supply of materials for the production of miasmata. This decomposition of vegeto-animal matter was in the present instance promoted by the intense heat of a scorching sun; and the me-

*See Professor Cooke's Essays on autumnal and winter epidemics in the 3d and 4th numbers of the Transylvania Journal &c. vol 1.

phitic gas exhaled from it, during the hot season, was, for a considerable distance, exceedingly offensive.

That this noxious effluvium was thus generated and its pernicious influence spread over the adjacent country, I cannot for a moment doubt. Col. N. J—'s and the Rev. T. A. S—'s families, in particular, felt the full force of this distressing fever. The latter gentleman has a large family of blacks in his possession living a mile and a half north-west of the lake: they enjoyed excellent health prior to the fall of 1826, although they had lived there for several years. In the summer of 1826 a large grove of timber, that grew between the parson's residence and the lake, was felled and deadened. This gave free passage to a wind blowing from the lake over his residence. Thus this febrific gas, the product of vegeto-animal decomposition, was driven by the wind into his family, and left among them a morbid predisposition to this perplexing malady.

The vast quantity of timber destroyed, as above mentioned, and the acceleration of its decomposition from the warmth and humidity of the atmosphere, must also have contributed to engender this pestilential miasm.

Col. J. lives much nearer the lake and on Flint creek. He has nearly 150 in family. Mrs. J., a very intelligent and observant woman, informed me that there were but three or four of the family who escaped an attack of the fever this summer or fall (1827). I have, myself, seen as many as thirty of the family sick at one time. Those cases nearest the creek were frequently the most malignant.

This creek (Flint) runs in a very meandering direction through this district of country. It generally ceases to run early in the summer; and, in places, contains a large quantity of stagnant water until winter. In the heat of summer this water is covered with a green scum, which affords a nidus for millions of insects, and the surrounding air is offensively tainted. Here again morbific emanations were produced.

For the three last years, 1825–6–7, the summers have been dry and hot, and bilious fever has in the main been confined to this neighbourhood. In 1826 and '27 scarcely a family, within

my knowledge, near the creek or pond, escaped an attack of fever; whilst the country, several miles distant, was generally healthy. Now, if climate is the only predisposing cause of fever, *why was this the case?* why was parson S—'s family exempt, comparatively, from fever till the fall of 1826? and why did they suffer immensely from it in 1827? I leave these questions to be answered by the anti-miasmatists of the day.

But we are told it is an axiom in ætiology, that a wet season is more sickly than a dry one. To a certain extent this is true; and, in my humble opinion, it is readily accounted for. Vegeto-animal decomposition goes on more generally, and miasma is produced in greater abundance. Is it contended "after a copious shower an epidemic seems to abate," and therefore malaria has no agency in engendering fever? We reply, that *heat*, as well as moisture, is a necessary constituent in the formation of miasmatic emanations; and that, if there be a sufficiency of rain to fill the ponds and prevent the stagnation of creeks, the epidemic will "abate" as a consequence, the cause being removed. Thus the process of putrefaction is checked and farther emanations of febrific effluvia are prevented. But if the showers be frequent and moderate, and the weather continue *hot*—which was the case, as I am credibly informed, in this country in 1823—miasm will be produced in great abundance, and fever is the consequence. The fall of 1823 was very sickly. In 1825 '26 and '27 the summers were *dry* and *hot*, as I before stated, and the principal sickness that came under my observation, was near stagnant water.

These, we hope, afford sufficient data for our conclusions, that fever *is* produced by malaria. In farther confirmation of this opinion, however, we hope to be indulged in a few additional remarks.

The last summer (1828) was sufficiently wet to prevent the lake, above spoken of, from drying up, and Flint creek never ceased to run but a few days at a time. This neighbourhood was more healthful than for the three last years. Now, let those advocates of the climatorial origin of fever throw off their gyves of predilection, and account for this satisfactorily. More-

ever, how does it happen that this neighbourhood was more sickly in 1826 and '27 than the country a few miles off? Perhaps it may be accounted for upon the principle of "variability of atmosphere;" as it has been urged, with some degree of seeming confidence, that because "one case of remittent bilious fever is produced" by a savage debauchery or a quixotic attack upon a "river," that, *a priori*, the existence of miasma is chimerical.

Are we told that caloric is a predisposing cause of fever? Then, dry hot summers ought to be generally sickly. The reverse of this is true. We are fully aware that *excessively* hot weather continued for a length of time, has a debilitating effect on the whole animal machine and produces perturbation of the biliary system. Hence, in the southern states, when a patient is attacked with fever, the liver is more apt than any other viscus to sustain the onus of the disease. Does this prove the non-existence of miasma? We are frequently told that the analyst has not been able to discover the nature of this gaseous fluid. But does not dire experience demonstrate most conclusively the existence of this subtle, invisible, and untangible poison?

Is the diversity of autumnal fever urged as an argument against the existence of morbific effluvia? We reply, that from the laboratories of putrefaction a pestiferous miasm is eliminated under different circumstances "of gradations of strength, as well as differing in kind;" and that the susceptibility of the system to its influence is greater at one period than another. The system may become inured to the impression of this effluvium, and the person is then said to be *acclimated*. Hence visiters to a sickly country, in the warm season, are more liable to an attack of fever than the permanent settlers.

ART. VII.—*On the use of the Bandage in the treatment of Various Ulcers, &c.* By BENJAMIN WINSLOW DUDLEY, M. D., Professor of Anatomy and Surgery in Transylvania University.

CONTINUED FROM PAGE 536, VOL. 1.

That remarkable epidemic which committed such dreadful ravages in almost every quarter of the United States during the latter part of the war, and for some time after, like the plague of the eastern countries, was strongly inclined to local determinations; in consequence of which many cases terminated in extensive formations of matter. Where these occurred among the muscles, they were generally converted into troublesome abscesses of the fistulous character. Several cases of this description came under my charge on my return to Kentucky in the summer of 1814; and from that period, throughout the years 1815 and 1816, the great characteristic of all bilious affections, (and there was no other form of fever,) was the strong tendency to concentrate morbid action on particular parts. This form of fever was so peculiarly distressing, and the local symptoms seemed to be so partially associated with, or dependent upon the general system, that some physicians did not hesitate to pronounce it beyond the reach of medical skill, and consequently in all cases fatal. From the summer of 1814 to the latter part of the year 1816, a large number of cases of this form of fever, in various stages of its history, came under my care. On my first visit many of them were found in a state of extensive suppuration, while in all, the tendency to excessive morbid action in the ankles, knees, legs, small of the back, wrists, elbows, or arms, was the most prominent symptom. In each case the progress to suppuration was invariable where it was not checked by the exhibition of active and efficient medicines. In one case abscesses formed in the fore-arm, leg and small of the back where I was not called on until the disease had become immovably fixed on these parts.

Many of those cases that suppurated which did not end speedily in death were, as before observed, ultimately converted into extensive and painful fistulous abscesses. It was in the management of one of these, in the summer of 1814, in a farmer of an adjoining county, that I tried, for the first time, the efficacy of the bandage; and the result exceeded my most sanguine anticipations. In March 1814 when the epidemic was extensively prevalent, this individual became the subject of a severe attack. After the fever reached its crisis a swelling was discovered on the inside of the thigh a little below the groin. When suppuration took place the matter dissected its way, by cellular absorption, as low down as the inside of the knee, where the integuments gave way and it was discharged. When I was called to see him, in July 1814, he had been confined to his back, on account of the abscess, with the leg gently flexed on his thigh, more than four months. There was extensive œdematous swelling of the limb, especially on the upper surface of the foot, on account of which, poultices had been assiduously applied with a view to promote the process of suppuration. The knee-joint, from inflammation and from being kept motionless, appeared to be destroyed, as it could neither be flexed nor extended.

The general œdema of the foot and leg was very much lessened by means of friction which was freely used. After rubbing the parts more than half an hour a roller was put on with firmness from the toes up to the knee. Another and broader roller was passed around the hips and brought down upon the affected limb. Graduated compresses, of sufficient thickness and magnitude to embrace the entire abscess, as well as the fistulous canal to the point of exit for the matter, were laid on and firmly secured by means of the roller which was brought down to the knee. The object of the dressing was to place the opposite sides of the abscess in immediate contact and thereby effect a union by adhesion.

In order to preserve the knee, provided anchylosis had not already occurred, the patient was required to leave his bed immediately and to take such exercise as was compatible with his remaining strength. Under the influence of the bandage the

swelling disappeared, and the abscess healed perfectly in a week. His general strength and health improved rapidly, but the treatment was adopted too late for the restoration of the knee, since that joint has remained motionless.

In September 1814, a youth in the vicinity of this place became the subject of a severe attack of intermittent bilious fever, with strong determination of morbid excitement to the small of the back and the joints of his limbs. In the absence of febrile excitement he was cheerful and free of all pain: but with the recurrence of each febrile paroxysm his sufferings were of the most agonizing character, in his elbows, knees, ankles and back. He had been sick fifteen days before I saw him. I found him with a large collection of matter extending from the knee to the ankle of one leg; while strong manifestations were given that the same course of morbid action would supervene in the other limbs and the back also. With each return of fever a degree of suffering, very seldom witnessed in any case, gave assurances to all around of the most imminent hazard of life; and notwithstanding the inflammatory nature of the case, it was thought absolutely necessary to combine liberal portions of opium with other medicines, in order to secure momentary tranquility, at the same time the most decisive means was resorted to, for the purpose of removing all causes of disease. So soon as by repeated emetics, mercurial cathartics, antimonial sudorifics and warm baths, the causes productive of fever were removed, the limbs and back ceased to suffer. The bandage which had been applied to the suppurating leg cured the abscess in five or six days after the constitutional disease was subdued, except in one small joint where the bone became diseased. The ulcer at that point was protracted until the bone was rendered healthy.

A servant boy was brought from an adjoining county with the most extensive fistulous abscess I have ever seen; it occupied the entire limb from the hip to the ankle. The disease succeeded to an attack of fever in the year 1816, and was of four months duration, when he was brought to me. Having resisted the measures usually relied on in such cases, it had been proposed to amputate the limb, and yet the extent of the suppurating sur-

face, in a species of ulcer hitherto tedious and too often intractable, was so appalling that this measure was finally abandoned; the more especially since the whole suppurating surface could not be removed. When the patient came under my care it was difficult to determine whether the symptoms indicative of a morbid condition of the digestive organs, or those confined more exclusively to the limb, were the most urgent in character. So soon, however, as an impression was made on his general system by medicines calculated to restore the healthy condition of the digestive organs, the more alarming symptoms attending the fistulous abscesses subsided: and these were perfectly cured by means of the bandage, before the general system was restored to health. At the expiration of six weeks he had recovered his natural complexion; his appetite and digestion became natural and regular; and he returned home perfectly healthy both in relation to his limb and general system.

I have already, in the 4th number of this Journal, alluded to an aggravated case of fistulous abscess in the leg and thigh, consequent to a fracture of the lower part of the leg, which was cured by three applications of the bandage, notwithstanding the patient was transferred to me after ineffectual efforts at a cure, persevered in for several months, and even when from the extent of the disease the effort at saving the patient by amputation was declined. I can with confidence add that I have never met with any difficulty in effecting a speedy cure in all similar cases.

Those fistulous ulcers which are produced and kept up by the presence of foreign bodies, constitute no objection to the use of the bandage; since in all such, it is equally desirable to control inflammation and swelling; and no other remedy which has been suggested is found to answer this purpose so well. Foreign bodies that penetrate deep beneath the surface, where by reason of the structure of the parts, or our ignorance of their precise position, all attempts at extraction are forbidden, may, from the quiet consequent to mechanical pressure, become closely embraced in a sac, and thereby rendered innocent; or where suppuration cannot be entirely controlled, it is attended by progressive ulceration which brings the foreign substance to the

surface according to a well known law which operates in these cases.

Where amputation is demanded, it might seem that the presence of fistulous abscesses running above the seat of operation would constitute an objection to such operation. It is however not valid. I have removed limbs where the stump was extensively permeated by fistulæ, without in any instance meeting with difficulty in the case, from this circumstance. Two years ago the limb of a patient was removed, whose knee-joint had been diseased for thirty years, and whose thigh was extensively dissected up by a fistulous abscess of several years standing; and yet on the twelfth day from the operation, and the first renewal of the dressing, which consisted alone of the bandage, aided by two strips of adhesive plaster, the abscess was cured and the stump cicatrized.

In 1821 I removed a gentleman's leg in consequence of an injury done the knee-joint by a fall from his horse. A fistulous abscess occupied the inside of the thigh; the knee-joint was destroyed by suppuration and the bones were extensively carious; hectic fever and great emaciation of person, with loss of appetite had induced the belief that this case was beyond control. Being some distance from the town I saw the patient for the first, and only time professionally, when the operation was performed. Directions were left not to disturb the dressings so long as they might remain well adjusted to the limb, and that the secretions, if any should occur, must be wiped away with a sponge. On the fourteenth day without any renewal of dressing this patient was quite well.

During the last summer I was called to amputate a man's limb in an adjoining county whose knee-joint had been destroyed by a disease of three year's standing, improperly treated. A fistulous abscess in the thigh extended considerably above the point where it was amputated. A single dressing cured the fistulous ulcer, and although the stump was not healed as early yet in the usual period it also was cicatrized.

A negro man, belonging to a member of the profession, was attacked with an acute affection of the hip-joint in 1816, attended

by some fever. For three weeks, as I was informed by the physician, his sufferings were uninterrupted, by reason of a concentration of morbid excitement on the joint. Indeed the local affection appeared to maintain such an exclusive monopoly of all morbid action, that the causes originally productive of his local disease were overlooked; and thus it became unmanageable in its nature. When suppuration took place I was called to see him. An extensive collection of purulent matter pointed near the anus which was discharged by a small opening. The patient, however, declined rapidly in his strength, his fever continued unabated, and notwithstanding every professional effort he died about ten days after the abscess was opened.

I have had many cases of a similar description since that period and also before, some of which had suppurated before I saw them, which were managed by the bandage,* especially where the matter had found its way extensively down the limb before reaching the surface. All fistulous abscesses of this description can be prevented by the timely use of the bandage, and by the prompt use of decisive prescriptions. Such cases are too frequently mistaken for affections purely local, and are thus, from improper treatment, rendered fatal in character.

Without regard to peculiarity, or to situation, the bandage, when properly applied, exerts a most commanding influence over all fistulous abscesses; and it invariably effects a cure without difficulty or delay, where it can be made to act on the seat of the secretion. The principal object of solicitude concerns the skill with which the application is made. When improperly put on, it is obvious that the bandage is mischievous in its tendency, as would be an inappropriate dose of medicine in any other case. In all cases where the disease is located in a limb, the entire extremity should be put under the influence of the roller. The seat of the abscess should first be embraced by the bandage, and then the fistulous canal and external opening in succession. The opposing sides of the secretory surface should

*Two years ago a gentleman published in London a solitary case of fistulous abscess in the thigh, which he cured by bandaging; and the treatment is recorded as a rare and valuable improvement in this department of surgery.

be brought into immediate contact, while the greatest pressure should be made directly upon the abscess, in order to secure it against the consequences of the pressure in the direction of the canal. When the seat of the purulent secretion is in the middle of the thigh, and the orifice for the exit of the matter is near the knee, a roller should first be put on from the toes to the knee, to secure the foot and leg from œdematous swelling; then another roller, wider than the first, should be brought down from the hips on the thigh, embracing compresses, placed directly over the abscess, of sufficient thickness and width to bring the opposing surfaces of the abscess and the fistulous canal all into immediate contact, from the pressure of the bandage, which is now to be brought down over the compresses to the knee. It is thus that the purulent secretion is immediately checked, while the whole secreting surface is made to heal by adhesion. When any doubt exists about the pressure being effectual, and where it is apprehended that a partial secretion may occur, the external opening should be preserved free, in order to encourage the exit of the matter.

With those unaccustomed to the bandage it might hardly seem necessary to apply pressure to a part of the limb more distant from the source of circulation than the abscess, and yet it is not to be dispensed with. Partial pressure will tend to obstruct free circulation in the more remote parts of the limb, and is in no respect favourable to healthy secretion and the closing of the abscess by adhesion, while it tends immediately to cause œdematous swelling. The dressing should remain undisturbed so long as it answers the purpose for which it was applied; but so soon as it becomes loose or irregular in its operation, a reapplication of it is necessary. The most mischievous consequences result from the removal of the bandage too frequently: and, not only in reference to the subject before us, but equally to all healing surfaces, it is a principle of great practical importance, to permit each dressing to remain as long as may be compatible with the ease and comfort of the patient.

Cases of fistulous abscess occasionally occur, wherein the free use of the knife is forbidden by reason of the extent and the im-

portance of the parts involved; and, because in such the less decisive means of treatment are insufficient, the patient is destroyed by a malady which might in a few days be corrected and cured through the influence of judicious pressure. In all the observations made on the superior efficacy of this mode of treatment, proper allowance must be made in those cases proceeding from caries and the presence of foreign bodies beneath the surface. Although the bandage cannot render the foreign substance innocuous, yet, as before observed, it checks in a very great degree all the consequences of morbid excitement proceeding therefrom.

The mode of action and the efficacy of the bandage are founded upon some of the most palpable principles in surgery. Artificial cavities that suppurate and discharge through an external opening must be destroyed either by granulation or by pressure. The surface that secretes pus is equally susceptible, under a change of circumstances, of furnishing the matter of adhesion. There is no incompatibility in the two grades of action, as very familiar observations made on suppurating and uniting surfaces very abundantly testify. The processes of suppuration and adhesion are constantly associated in the surface of ulcers that are healing. Opposing secreting surfaces when placed in contact, are capable of union by adhesion without any observable change in the state of the parts. Although very different in their results, yet these processes occur upon the same surface.

Notwithstanding the treatment of fistula in ano does not involve directly the principle of treatment by the bandage, yet there is a fact in its history highly corroborative of the influence of pressure in the cure of all fistulous abscesses. This peculiar abscess being immediately under the influence of muscles that are ever active, and that are engaged successively in drawing the soft parts in various directions, it is obvious that in its treatment some means must be resorted to, capable of preserving the muscles in a state of tranquillity, without which a cure cannot be rationally anticipated.

Fistulous abscesses, however extensively they may involve the rectum, are susceptible of a speedy cure by dividing the

constrictor ani, with only so much of the bowel as is embraced by it; and although the disease may run parallel with the bowel and communicate three or four inches within or above the constrictor, still the treatment is the same. Nor is there any variety or any additional treatment called for, in cases of double fistulæ; inasmuch as it is only necessary to suspend the actions of the constrictor and of those muscles connected with it, in order to place the parts in a condition most favourable for a cure. The great efficacy of the bandage, in the treatment of all fistulous abscesses, consists principally in maintaining the muscles concerned, in a state of repose. In reference to fistula in ano since it is impossible to apply pressure successfully, it is obvious that the division of the muscle constitutes the only resource with the practitioner. When the constrictor ani is divided, the soft parts of the vicinity are placed thereby in a state of rest; a condition which is followed by a reduction of the indurated state of other fistulous affections of the same part, where they exist, while healthy suppuration and adhesion speedily take place in all. It is true the condition necessary for restoration is secured at an earlier period in the sinus operated on, than elsewhere; it does not occur however with more certainty, under judicious management, than in the sinus more remote. Simple incised wounds are inclined to heal with much more facility and with less delay than indurated ulcers; but the causes of induration being once removed, these will not fail to heal with as much certainty as a clean incision.

The common sore leg, for the improved treatment of which Mr. Baynton has justly obtained a high reputation, presents considerable variety in its aspect, and requires close attention to the mode of applying the dressings. In all cases pressure by adhesive plaister and the bandage is proper; but the degree of force to be used in applying it must correspond, in each case, with the excitability of the constitution, the health and energy of the system, the degree of exercise taken by the patient during the cure, the history and protracted nature of the ulcer, the sex and age of the individual. A dressing which would answer in a patient of an excitable or inflammatory habit, confined to

bed or to the room, would prove prejudicial under the influence of exercise, by exciting the absorbents to ulcerative action. A bandage applied with firmness upon the ulcerated leg of a patient labouring under fever, would excite ulcerative absorption, and thereby extend the dimensions of the ulcer, which under precisely the same treatment, with a tranquil and healthy state of the general system, might have tended most rapidly to a cure. A simple ulcer in an aged individual admits of firmer pressure than an ulcer of the same species in a young subject. The firm fibre of the male calls for a higher degree of pressure, in the treatment of ulcers, than the soft texture of the female. In all, as well the young as the old; the delicate and infirm, as well as the strong and robust; the active and laborious, as well as the sedentary and secluded, the principles of cure are the same. The approximation of the edges of the ulcer, the suppression of exuberant and the promotion of healthy granulations are equally desirable, as the mode of cure, in every case.

Although I have recommended pressure as superior to all other treatment in the cure of simple ulcers on the extremities, yet I dissent from Mr. Baynton's mode of using it; inasmuch as very many cases do well without the aid of adhesive plaister, while every one would do better under a different application from that recommended.*

Applied according to the rules laid down by Mr. Baynton the adhesive strips produce the effect of a common ligature on the limb. But besides obstructing cutaneous circulation a strip of adhesive plaister, made to encircle the limb, does not combine the utmost advantage in approximating the edges of the ulcer. On the contrary, a piece which will pass about two thirds around the limb will command a much larger amount of integument, by which to diminish the surface of the ulcer, at the same time that the circulation is left more free and uninterrupted.

For these reasons I have been accustomed for many years, in

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* Mr. B. recommends adhesive strips about an inch broad, and three inches longer than the circumference of the limb where it is ulcerated. These are to be applied directly over the ulcer and for one or two inches above and below it.

dressing ulcers on the legs, to use adhesive strips that pass only about two thirds the circumference of the limb; taking care to approximate the edges of the ulcer at the time they are applied. The common roller then gives support to the entire limb, and maintains the adhesive dressing securely in its place.

But while the bandage is admitted to be a most valuable improvement in the treatment of common sore legs, it will be found to be far more important as an application to indurated ulcers; to those with inverted edges, with thickened indurated edges; to those attended by fungus, and to every species in which there is an enlargement of the limb, either of the indurated or œdematous kind.

The practice so universally inculcated of destroying the edges of an ulcer, because they are thickened or inverted, takes its origin from false views in regard to the causes which obstruct the healing process. In place of considering the indurated or inverted edge of the ulcer, as a morbid condition to be removed by cautery, the scalpel or blisters, preparatory to the cure, it is a condition which when properly understood will be hailed as a happy manifestation, that nature under auspicious circumstances is still competent to the task of her own renovation. From what cause is the sound skin surrounding an ulcerated surface inclined to dip into the ulcer and hide itself beneath? or to become thickened and thereby present elevated and indurated edges around the ulcer? It must be obvious to those who reflect properly on this subject, that there can be no diseased disposition in the surrounding integument which could invite it either to dip into the surface of the ulcer, or to raise a living wall between it and the adjacent healthy parts. On the contrary it is most manifest that these are two modes of fortification, resorted to by nature, in order to prevent the extension of ulceration in all those cases, wherein, from the site of the ulcer, or the morbid condition of the general system, the healthy processes subservient to restoration are suspended.

These states of the surrounding integument are never seen except in ulcers of long standing. When nature is incapable of prosecuting the work of restoration, whether from local or

general causes, a precaution is taken to prescribe certain limits to the ulcer. This consists in an inflamed state of the edges of the ulcer, whereby the integument is either thrown up as a living wall, or runs insensibly into the texture of the ulcerated surface, presenting thereby a barrier to its extension. In either case, the skin is for the time immovably fixed; nor can ulceration ever extend except so far as this living rampart is broken down. These conditions of the integument surrounding a chronic ulcer must, therefore, be received as healthy manifestations; nor can I conceive how the intelligent in the profession can withhold assent to this view of the subject, when the strong corroborative fact is adduced, that any treatment, either constitutional or local, which tends to restore the edges of the skin to a natural state, will be found immediately conducive to the cicatrization of the ulcer. The destruction of the edges of an ulcer, by cautery or otherwise, as preparatory to healing, is like the practice of the empyric who converts all intractable ulcers into burns, under the impression that all recent burns heal with facility.

Since then there is nothing morbid in the integument surrounding ulcers of long standing, but, on the contrary, such a state of the skin occurs as secures the surrounding surface from ulceration, the practice should no longer be listened to of destroying it preparatory to healing. In many cases of this description of ulcer, the condition of the limb is quite as unfavourable as the state of the ulcer itself, to healthy granulation and cicatrization; and consequently, although the ulcer may be partially prepared for healing by the inflammation consequent to the destruction of its edges, still the morbid condition of the limb would be sufficient to prevent a healthy and permanent cure of the case.

From all that has been said it must be palpable that a remedy calculated to restore the healthy condition of the limb, to reduce its hardness, to discuss all enlargement, to give support to the blood-vessels, to change the actions of the ulcer, and to prepare it for healing, is not to be found among the applications commonly relied on in these cases.

That which experience will sanction as most successful in all

solutions of continuity, will be found to constitute almost the exclusive remedy in every species of ulcer under discussion; while the extent of its application and the period of its continuance, will be directly in proportion to the morbid condition of the limb and the magnitude of the ulcer.

The liberty enjoyed by the patient in the use of the limb should be regulated by the character of the ulcer and the condition of the leg. The exercise should be moderate, provided there be neither induration nor enlargement of the leg. On the contrary, encouragement should be given to take free exercise in cases of enlargement, and especially when this is accompanied by induration. The bandage, aided by muscular contraction in exercise, will facilitate the reduction of all hardness and swelling in the leg; while a cicatrix formed under these circumstances will be much less liable to subsequent ulceration. In the simple species of ulcer, much exercise, under the pressure of the bandage, will frequently arrest the progress of the cure in very excitable constitutions. A great error has been committed here also in dressing ulcers too frequently: by this practice the tender surface is injured, the small vessels are broken up, and the newly formed adhesions are destroyed. There is, indeed, no principle in surgery entitled to a more unqualified admission to the confidence of the faculty, than that which points out the propriety of dressing ulcers as seldom as possible, having regard to the comfort of the patient. The secretions never prove irritating to the surface from whence they are derived; hence the dressings should only be renewed when they cease to answer the purposes for which they are applied, or demanded by a due regard to cleanliness.

A gentleman, about sixty years of age, applied to me on account of an ulcer on his leg which extended four inches around, and about ten inches in the longitudinal direction of his limb. The ulcer proceeded originally from local violence and was subsequently aggravated by an attack of fever. For thirty years he had been successively the patient of many of the physicians and most of the empyrics of the western country. In 1816 he became my patient, at which time the limb was as hard as a pine

board, of twice the proper magnitude; the edges of the ulcer were inverted to a great extent, the surface of it having extraordinary morbid firmness, and pouring out an excessively offensive secretion. The necessity of exciting the action of the absorbents with a view to reduce the magnitude, as well as the induration of the limb, was suggested by the first examination. For this purpose he was ordered to take mercurial cathartics every other night, to use warm and steam bathing daily to the limb, to wear a roller drawn with as much force as could be conveniently applied, to live on a diet of bread, milk and vegetables, and to use exercise freely on foot.

For the first three weeks no very remarkable change was perceptible in the appearance of the leg except in point of induration; but after that time the daily influence of the treatment became matter of observation. At the end of the fourth month, without the aid of any additional dressing, the leg was reduced to dimensions something less than the sound limb, and the ulcer had entirely healed. The course of light dieting, with cathartics, was continued through the greater part of the treatment. After the first month the bathing was discontinued, and the bandage was removed, for the purpose of tightening it, two or three times a week. When the ulcer was cured, precautions were taken to prevent any unpleasant consequences from healing a protracted ulcer, by requiring of the patient to use mercurial cathartics twice, and lastly once a week, for several months in succession; and to continue to support the blood-vessels of the limb by means of the bandage. This patient enjoyed himself for ten years after; taking care in case of fever or general indisposition to use medicines promptly, and to favour the weak limb in order to prevent disease from being located there.

It is quite as difficult, and costs much more time to remove predispositions than to cure diseases; hence the liability to a return of chronic disease. In all similar cases it is absolutely necessary either to rely on free exercise on foot, in aid of the bandage, as one of the best means of promoting interstitial absorption, or to apply the bandage and confine the patient unconditionally to a horizontal posture, using mercurial purgatives and re-

stricting him to a meagre diet. The interstitial absorption which ensues upon tedious confinement, aided or not aided by medicine, will exert an influence on most indurated swellings, and especially upon those attended by, or consequent to, ulceration of the ordinary description.

In the memoirs of the French Academy of Surgery of Paris a large volume is occupied in discussing the virtues of the various dressings for ulcers; and, in obedience to the practice of all authority in favour of the various poultices, cataplasms, cerates, and washes, Sir Astley Cooper recommends, according to the result of his own experience, a singular succession of these, in the dressing for ulcers of a particular description. According to Sir Astley Cooper the whimsical character of some ulcers constitutes the most prominent feature in their history; while there is no criterion by which to anticipate the change, or to discriminate the appropriate remedy for the moment. It is on this account that he thought it the more necessary to call the attention of the profession to a variety, in order that some one of those suggested might always be found to answer for the time. Hence the fermenting, the carrot, the wine, the spirit, the bread and milk, with a variety of other poultices, unguents and washes, resorted to without principle, applied without discrimination and relied on without any assurance of the efficacious influence of either. With those who have witnessed the remarkable difference between the atmosphere of London and the adjacent country, on the constitutions of the sick, the difference between the air of the city generally and of the large hospitals, and especially of the wards appropriated to cutaneous and ulcerating diseases, where the atmosphere is frequently so charged with pungent effluvia as to irritate the eyes and nostrils of the visiter, it is not matter of surprise that Sir Astley Cooper should be engaged in multiplying the remedies in ulcerating diseases. Through the channels of information of the time, the public are taught to look upon particular species of ulcer as the most capricious diseases in nature; since without any particular appearances indicative of the propriety of certain dressings, they are seen to heal and to ulcerate under the influence of an

application most judiciously selected, while that which will answer better is not made known by any manifestation of an unerring kind.

Such cases as these concur to affix an inestimable value upon Mr. Abernethy's paper on local diseases proceeding from constitutional causes; since whatever the causes producing them, the painful symptoms being of constitutional origin, are to be controlled by addressing remedies to the general system; in confirmation of which we may remark that an emetic has a more soothing influence in the painful irritable ulcer, than all the local applications which have been suggested by different authors.

The irritable, the inflammatory, the fungous, the varicose and the sloughing ulcer, however diversified the causes productive of them; and, notwithstanding the dissimilarity in their manifestations, are equally under the controlling influence of pressure; so far as local applications can be made to produce any sensible impression. A painful irritable ulcer cannot be successfully treated except by the aid of constitutional remedies. It is in the condition of the general system that it takes its characteristic, and consequently when the morbid condition of the system is corrected, the ulcer is left in the state of one of the most simple species, to be cured after the manner of Mr. Baynton, or rather by properly adjusted mechanical pressure.

The inflammatory ulcer presents no additional embarrassment to the practitioner in the treatment by the bandage. Whether proceeding from constitutional derangement, or from the isolated condition of the limb, the beneficial effects of this application are equally demonstrable.

It is not unknown to the writer, that, in disordered conditions of the general system, and especially where the constitution is of an excitable character, the incautious use of the bandage will excite ulcerative absorption in ulcers of the irritable and inflammatory character: but this constitutes no objection to its use; since in skilful hands it cannot fail to do good. The patient whose ulcer, in consequence of the deranged condition of the system and the inflamed or irritable state of the ulcer itself, is inclined to extend itself under the influence of the bandage,

whilst the privileges of exercise are allowed, will as certainly secure the conditions requisite to a speedy restoration, provided the system be correctly treated and the patient ordered to a state of repose. It would appear, then, that these, as well as other species of ulcer, are calculated to establish the superior efficacy of the bandage, while they inculcate most strongly the necessity of turning more attention to the influence of the general system or local diseases.

The circumstances upon which the processes of ulceration and cicatrization depend, do not appear to be entirely understood by the majority of those authors who have written on the subject. Quite too much attention has been bestowed on the various local means, with a view to accomplish the purposes of healthy granulation, as well as to correct the morbid action of the absorbents in ulceration. Unhealthy, deficient, as well as luxuriant granulations are too commonly attributed to the exclusive influence of causes local in character, and subject to the influence of local remedies. Hence the endless variety in the dressings resorted to in cases of the kind in question, as recommended by every author who has written upon them. But it appears evident that two causes only can contribute to any unhealthy manifestation on the surface of an ulcer. One of these is to be found in the general system, the other in the condition of the ulcer itself, independent of constitutional influence. Luxuriant granulations more generally proceed from a strong effort in the ulcer to heal: at other times they are the result of preternatural excitement of the general system, located in the ulcer. Sometimes the granulations upon the surface of the ulcer become preternaturally firm; more frequently they are soft and spongy; and occasionally they prove too weak to sustain themselves, in conseqence of which they are absorbed, and the ulcer assumes a ragged, irregular appearance.

In all these cases the bandage is manifestly the best local application that can possibly be made. By its influence the growth of profuse granulations can be immediately checked; when preternaturally firm, as in chronic ulcers, they are rendered soft and thereby put in a condition for healing; when too weak

to sustain themselves, by approximating the edges of the ulcer, the bandage diminishes the granulating surface and thereby communicates strength to what remains. Those granulations which are irritable, and have been formed under the influence of a morbid state of the general system, are ephemeral under any treatment; and while the bandage may excite ulcerative absorption in these cases, it always ceases so soon as the general system is corrected; and without this it is impossible to manage the case by any local treatment. The bandage is a substitute for the natural integument, and in skilful hands must succeed in all cases of ulcer, not specific in character, when care is taken of the general system.

The varicose and sloughing ulcers are more especially under the control of mechanical pressure. Without the bandage we have no means by which to manage varicose ulcers, while no case of this description can resist its healing influence. I have seen a leg swelled to three times its proper magnitude, with an ulcer as large as a half sheet of letter paper, restored to its usual size and health, in twelve days, by the use of the bandage. Although this remark may startle credulity, yet it is believed that it will not be esteemed extravagant by those who understand, that very extensive varicose ulcers are frequently encountered by the practitioner, in which the entire texture of the skin is not destroyed. Indeed, of all the cases in surgery in which this remedy is commended, there is no one in which it is more speedy and more commanding in its influence, than in varicose ulcers.

The sloughing ulcer is more frequently attended by an exhausted condition of the general system; sometimes it is the result of great violence done to, and exposure of the ulcerated surface. In either event mechanical pressure is more happy in its influence than any remedy hitherto suggested. The influence of no principle in surgery is more demonstrable than the efficacy of the bandage in putting an immediate check to inflammatory action and consequent mortification in a wound; nor is the certain efficacy of this agent, in any respect, lessened in those cases where mortification has already supervened: on the con-

trary, it is here that its benefits are probably most manifest; since it suddenly checks excessive fetor, diminishes the magnitude of the limb, contracts the dimensions of the ulcer, checks sanious discharges, and promotes healthy suppuration. While it arrests the progress of mortification it hastens the process of separation between the dead and the living.

In cases where great violence is done, with destruction to the soft parts, I have witnessed, by the immediate application of the bandage, a separation of the living from the dead parts, with a fine suppurating surface void of tumefaction, on the fourth day from the injury. Even under circumstances the most unpromising as in the tearing up of the connexion between the carpal and metacarpal bones, with a destruction of all the soft parts in the palm of the hand, a separation by healthy suppuration, has been completed by the fourth day, and a cicatrix on the carpal bones by the fifteenth from the injury.

It may seem rather paradoxical that a remedy so efficient in checking inflammation with consequent mortification, should be commended in language equally unqualified for the treatment of mortification proceeding from an opposite condition of the general system and of the limb. It is however as explicable on rational principles as it is important in its practical application. In all cases of mortification, proceeding from a prostrated state of the general system, a tumid state of the parts involved in the local affection is a characteristic feature; and in all these a well adjusted bandage, by sustaining the blood-vessels, by promoting venous absorption, by preventing too great an accumulation of blood in the parts, will change the condition of the ulcer, and prepare it for healthy suppuration. On the contrary all poultices and other dressings that invite an increased enlargement, most certainly tend to aggravate the danger of the patient.

(TO BE CONTINUED.)

ART. VIII.—*A Case of Chronic Disease of the Brain, relieved by an operation with the Trephine.* By DR. WILLIAM JUDKINS, of Mount Pleasant, Ohio.

(COMMUNICATED BY PROFESSOR CALDWELL, WITH THE ANNEXED LETTER.)

J. W. a young man about eighteen years of age, of rather a delicate constitution, and never very sharp-witted, (to use a common phrase,) though scarcely observable unless to those well acquainted with him, for the last twelve or eighteen months appeared at times in a state of religious melancholy. The family often observed him inclined to retirement and having very little to say to any one; and sometimes, when at meeting, he would utter a word or two by way of exhortation. When about home in his common walks he had a downcast appearance and very little to say; though he would work on the farm, with his father and the other hands, in the ordinary manner. His disease evidently increased upon him, and his parents growing uneasy at his situation, his mother came with him to our office in order to have some advice in his case. This was in the summer of 1823; and in addition to the symptoms above mentioned, he was now labouring under fever in the evenings, pain in the head, slow, full pulse, furred tongue, dilatation of the pupils, listlessness, cold feet, and a kind of tardiness in moving from his seat.

From these appearances we were led to suppose that inflammation of the chronic kind had located itself in the membranes of the brain; and under this impression he was freely bled; a seton introduced into the back of his neck; purgatives and low diet enjoined, together with cold applications to his head. In about two weeks after this prescription we were called upon to visit him. We found him mostly confined to his bed, low fever, pulse slow but not full, dull heavy pain in his head, tongue a little furred, and at times some mental alienation. On ascertaining that the prescription heretofore given had made but ve-

ry little impression on the disease, and seeing that a fatal issue was fearfully to be anticipated, the idea of relief was very much involved in doubts. The case had, altogether, a direct tendency to call up a former case of the same kind which I had treated, with all its trying and unyielding difficulties and fatal issue.

After pursuing the depleting plan as far as the system would bear it, without relief, and this too in every possible way, calculated to overcome the disease, we now concluded to place him under the influence of mercury. This we did, and continued it as long, and carried its effects as far, as prudence would warrant; but all to no effect. What was to be done now? here was the routine of treatment gone through faithfully, in all its parts, and the patient not at all relieved. In consultation it was believed that his disease was out of the reach of medicine, considering the patient's low and debilitated situation.

He was by this time in a state of utter insensibility, and as much emaciated as it is common to see any person living. When food in a fluid form was put to his mouth, he would take a small portion of it; he made very little complaint; lay with his eyes wide open; his alvine and urinary discharges were involuntary; and when his limbs were laid in any ordinary position, he would not move them for twelve or sixteen hours together.

I now proposed to my medical friends to carry the treatment one step farther; and that was to try the effects of trepanning. This was objected to, as a very doubtful and unprecedented remedy; and as the patient, to all human appearance, would soon expire, it was for the present suspended. He continued in this situation for several days, without any material alteration. I again proposed and insisted on my former proposition, which was at length consented to. His parents were now informed of and made acquainted with our intentions, together with the nature and tendency of the operation, as well as its doubtful result; that it would give their son one further opportunity for recovery, though a questionable one; that it would not shorten his days in the least; and, as he was in a state of utter insensibility, there was nothing to dread from any pain on his account.

Without the least hesitation they consented and desired us to proceed to operate.

In company with two of my medical friends, I removed a portion of the parietal bone with the trephine, and found the dura mater marked with a slight tinge of chronic inflammation, and the brain pulsating slowly, and tolerably firmly beneath. There was not more fluid than natural between the cranium and dura mater. I dressed the wound in the usual way, except that in replacing the flaps, I removed a corner of one of them, in order to keep up a direct opening with the dura mater, as I expected the absorbents of the brain might be invited to an increased exertion from the irritation in this particular part, and be thereby solicited to unload the vessels.

Our patient bore the operation much as a subject would, on the table in the anatomical dissecting room. I saw him the next evening and, to my great satisfaction, found that a sanious fluid had escaped from the wound, (I might say from the brain,) and had run through, not only the pillow under his head, but also *through the bed:* and there were other symptoms which excited my admiration. His speech and the exercise of his mental faculties, which had been entirely suspended for the last six or eight weeks, began to return. This was a state of things very much desired, but could hardly have been expected so soon after the operation. From this time he slowly recovered, and has entirely regained his health and strength; and, what is rather more remarkable, he is *much more acute in the exercise of his mental faculties than he ever was* before the operation with the trephine.

To the Editors of the Transylvania Journal of Medicine &c.

Gentlemen, The accompanying case, which I have just received from Dr. Judkins, and consider full of information and interest, I shall be pleased to see published in the Journal you conduct. It is, in a high degree, creditable to its author, as a practitioner at once judicious and bold; not satisfied with common remedies in desperate cases, but sagacious to devise means for the preservation of life, and intrepid to exe-

cute every thing practicable to that effect, even at the hazard of his own reputation.

The case shows not only how the brain may be relieved from a very threatening and apparently desperate affection, but how it may be *improved*, as the organ of the intellect. In the latter point of view, although it is striking and important, it is not new. There are on record many instances, in which an alteration in the tone and general condition of the brain, in consequence of mechanical violence, has greatly increased both the strength and activity of the intellect of the individual. I have good reason to believe, that, in the present case, the intellectual change was much more striking than Dr. Judkins represents it.

A gentleman now attending the lectures in the Medical School of Transylvania, was a pupil of Dr. Judkins, about the time of the operation and cure performed by that gentleman. He had also an intimate knowledge of J. W. both before and after the operation. He knew him at school, and permits me to say, on his authority, that he was so exceedingly feeble and dull in intellect, as to be scarcely able to learn to read; that in reality he could not learn to read correctly; and was equally incompetent in other branches of knowledge. But since his recovery this gentleman assures me that his intellect has been of an order entirely different. His capacity is now good, he converses intelligently, his countenance, which was before dull and inexpressive, is sprightly and animated, and instead of being heavy and comparatively feeble, his movements are sufficiently lively and vigorous.

It is known that I have long asserted my belief, that various affections of the brain, including madness, may be often removed by surgical operations. The cure, by Dr. Judkins, is in proof of this opinion. So are those of epilepsy effected by Professor Dudley, and published in your Journal.

In speaking of the probable importance of the trephine, in the treatment of diseases of the brain, my allusion has been chiefly to madness. When we shall have so successfully studied that complaint, in all its modifications, as to know in what particular parts of the brain the morbid affection, productive of its various forms, is situated, I feel persuaded that the trephine will be used in it, with the happiest effects. In the mean time, permit me to say, that for further improvement in the science and treatment of insanity, we must be indebted chiefly to the study of phrenology, healthy and morbid.

December 29th, 1828. C. CALDWELL.

ART. IX.—*A Case of Hæmorrhois cæca, cured by the insertion of Setons.* By RANSOM MALONE COLLINS M. D. of Feliciana, Louisiana.

IN the treatment of no class of diseases, have I discovered greater difficulty than in the different hæmorrhoidal affections; and the frequent failure of the practice recommended by the books, in the removal of these affections, at once not only declares their difficulty, but the necessity of more attention being bestowed on this subject. With this view I offer to the consideration of the profession the following case, inasmuch as I have seen in no work the recommendation of setons, for the cure of piles.

In the spring of 1827 I was consulted by S. D. a man of about 34 years of age, of good constitution and athletic frame, for what he supposed an affection of the kidneys; having been so informed by his physicians, prior to his visiting me. He informed me that about eighteen months previously he had laboured under a dysenteric affection; and that on its disappearance, he was continually distressed with a dull pain in the region of the kidneys, playing backward and forward, to the anus, creating a desire to go to stool very frequently, and a like desire to void urine. His stools when not under the influence of cathartic medicines, were scanty and of a semi-gelatinous consistency and complexion; his urine was voided in small quantities, of a very high colour, and for the last three months before I saw him, its discharge was attended with great pain at the neck of the bladder. This pain, which at first was only felt when discharging the contents of the bladder, soon became constant, and so very severe that at no time could he enjoy any respite, except while lying with the lower part of the abdomen across the top of a chair.

I was induced to believe his case was hæmorrhoidal, from the circumstance of its having been ushered in by a dysentery,

and that the affection of the urinary organs was secondary, or rather sympathetic. I so informed him, and gave him some general directions, with an expectation that he would remove to a situation where I could attend him constantly. He, however, placed himself under the care of my friend Dr. P., who put him under a mercurial course, together with the routine practice in such cases, without affording him any relief. His case was now considered hopeless, and so pronounced. He remained "in statu quo" or getting rather worse, until the 19th of November, when I was requested to visit him. On arriving I found him lying across a chair, and in great agony. I immediately made an examination per rectum, and found my conjectures had been right; for the whole of the intestine, as far as I could reach, was thickly studded with small tumours, with a very considerable enlargement of the prostate gland. He was now much emaciated, labouring under a continued fever and no appetite.

Being aware that the course recommended by the books had been unsuccessfully pursued in his case, I resolved to try the counter-stimulating plan by the insertion of setons. This I did by introducing one on each side the os coccygis, a small distance above the verge of the anus. He was directed to use, as an almost constant drink, molasses and water, and to make his diet entirely of molasses and mush or rice; with the occasional use of super-tart. potassæ.

On the 26th I visited him, and found the setons discharging freely; made an examination per anum, and had the satisfaction to find that the tumours were disappearing, the patient being much better in every respect.

December 3d. On examination I found the tumours all gone, except two or three; he had urinated freely for the last three days; had no inclination to lie across the chair, and his appetite was much improved.

On the 14th December the patient says he is well, being able to attend to his business; I directed him to continue the dietetic course prescribed, and to wear the setons for a considerable time yet. These were not removed until sometime in Janua

ry, when all symptoms of his disease had vanished, and he has remained entirely exempt from any attack until this time.

I have had no opportunity to try the efficacy of this course since, but am fully of opinion that in all cases where the disease does not depend on an obstruction in the liver, to the free return of the blood from the inferior extremities, that this course will succeed, in a shorter and more effectual manner, than any I have heretofore tried. I would at least be glad to have it fairly tested; for I must acknowledge that I have been often mortified at a want of success in the treatment of these affections.

December, 1828.

MISCELLANEOUS INTELLIGENCE.

Reduction of a Dislocation of the Hip-joint of four and a half months duration.—One of the editors has received a letter from Stephens R. Beatty, M. D. of Clear Spring, Washington County, Maryland, giving an account of this interesting operation. We take the liberty of making an extract from his letter; under the hope, however, that Dr. Beatty will favour the public with a more detailed history of the case.

"I have lately reduced a chronic luxation of the femur into the foramen thyroideum of four and a half months duration, in a boy of sixteen. Although I have not succeeded in keeping the head of the femur permanently in the acetabulum, the case has established an important surgical fact, *that even in a youth of sixteen the acetabulum is not so much filled up at four and a half months after dislocation as not to admit the head of the femur*, it having entered with an audible snap. Its being the downward dislocation, (in which there is nothing to overcome but the preternatural adhesions, and a few muscles, the gemini, obturator externus and internus, and quadratus femoris; which an examination of the skeleton, with the femur in the head of the acetabulum and in the foramen thyroideum, will prove to be shortened in this dislocation, all the rest being elongated,) inspired me with confidence. I bled my patient standing, from an orifice in each arm at the same time, and gave him tartar emetic, ad deliquium animi, when he was laid upon

the table. My pulley multiplied six times; two men were required at the rope; in one hour and a half the head of the bone entered the acetabulum with a loud snap, when the malleoli and knees were found to be opposite; a fracture of the neck of the femur entered my mind, but the disappearance of its head from the groin and the subsequent history of it dissipated all doubt. I confined the legs together with the bandage, and placed my patient upon his back on a firm bed. Having a patient four miles in the country very ill, I left the former for a few hours in care of the nurse, with particular cautions as to quietude. Being a boy careless of consequences and having a careless nurse, I was mortified to find on returning from the country, two hours after, that in his restlessness he had thrown himself around on the trochanter of the injured side, had flexed the thighs upon the pelvis, that the knee of the dislocated leg was again pushed in advance of the other, notwithstanding the bandage, and that the head of the femur was again in the groin.

I consulted Doctors Dorsey and Hammond of Hagerstown, the two oldest medical gentlemen in the county, having considerable surgical reputation, on the propriety of replacing the bone as the adhesions were all loose, and making use of other means for retaining it. On visiting him they agreed with me in the propriety of it, and Dr. Hammond proposed using Dr. Physick's long splint, with a band to act from it upon the neck of the bone to keep it up, and another to pass over the opposite shoulder and keep the splint up so as to counteract the first one; and likewise to confine the legs together. But all we could say could not prevail on his friends to allow us to disturb the limb again. Considerable inflammation and constitutional disturbance followed of course. Having some chylopoietic derangement, the consequence of intermittent fever last fall, (1827), I had taken care to prepare him with tartar and calomel for about a week, when the biliary system became perfectly healthy. To this precaution, for which I am indebted to Professor Dudley, I must ascribe his present safety. All the inflammation of the hip, and most of the constitutional disturbance have subsided. The motion of the joint is as free as it was before, and I think in a short time he will be able to walk about.

Perhaps I have been too bold in attempting this case, but I expected to succeed, and I think I should have done so, had I not have had such ignorance to contend with. The plan suggested by Dr. Hammond would probably have succeeded; yet it is doubtful, as the transverse ligament must have been entirely destroyed, and the acetabulum was no

doubt somewhat filled; it might therefore have been impossible to keep the head of the bone from passing out by any means; but could it have been done, the pressure of the head on the deposit in the acetabulum would have caused its absorption and rendered it again perfect."

Utility of the Bandage in wounds of the arteries.—Testimonials of the superior utility of this powerful agent, in the relief of various diseases and accidents, are constantly accumulating on our hands. Our limits will only permit us, at present, to give the following abstract of a very interesting case of a wound of the ulnar artery near the elbow, attended with great swelling, mortification and other discouraging circumstances, which was nevertheless successfully treated by Drs. N. Gaither and Winston of Columbia, Kentucky. The case was communicated by the former gentleman to Professor Dudley, the warm advocate of the practice. We need scarcely add that until very lately this mode of treating inflammation and sloughing was unheard of.

"John Dungins, aged thirty, a blacksmith by trade, was accidentally wounded in the right arm with a large butcher knife, in December 1827. It entered just below the internal condyle, passing obliquely downwards across the arm, about three or four inches, dividing the integuments and muscles to the bones, with the ulnar artery below its junction with the basilic. Local pressure was applied by his friends, with temporary effect only, the hæmorrhage returning every two or three days, until the 11th day, when I was called in. I found his system very much deranged from the loss of blood and inattention; a pallid cadaverous appearance of the face, and great weakness; so great was his exhaustion that he could not be raised up without fainting; but much action of the heart and arteries remained, except in the radial artery of the wounded arm, which had no pulsation. The hand and arm together were much swollen, with a cold surface and gangrene in the wound.

"The swelling was so great and the health so bad, that they forbade the attempt to take up the artery, for fear of mortification and secondary hæmorrhage. Amputation was still less inviting; and consequently the use of the bandage was thought the best remedy by Dr. John D. Winston, who accompanied me, in which opinion I heartily concurred. The arm was therefore, well bathed with warm spirits and the roller applied from the hand to the arm-pit, drawing the wound together with adhesive strips. Alteratives with an anodyne were given

and the patient left, with directions to tighten a cord, which was put on the upper arm, if bleeding ensued, sufficiently to stop the circulation until we could be sent for. In twenty-four hours the swelling was so reduced that the bandage became loose, and the bleeding recurred: it was stopt by the cord until the bandage was re-applied; seven days after it was again applied for the last time, and in 25 days from its first application he was able to make an axe.

"I have managed many cases of injury with the bandage with, (I must acknowledge) unexpected success, but experience and the high authority of its utility as a remedy has dispelled all doubts. Many cases of erysipelas of the limbs have yielded to its powers, and although strong prejudices exist against the practice with the bandage, time will remove them all."

Fracture of the os humeri, with separation of the Condyles.—In confirmation of the views of Professor Dudley in regard to the practicability of effecting a union by bone within the capsules of joints,* we add the following case, communicated to him by Dr. E. Coons, late of Moulton, Alabama.

"Since I wrote to you, in the summer, on the beneficial employment of the bandage in *erysipelatous inflammation* of the extremities, I have had occasion to resort to it, as a chirurgical agent, in a fractured os humeri. The case occurred in a boy abont 10 years old. He was thrown from a horse a mile from town, on the root of a stump. I was immediately sent for. When I arrived, his arm was bent considerably backwards, and the end of the bone seemed to project nearly through the integuments, which were greatly contused and of a very dark colour, from the extravasation of blood. Upon further examination I discovered the bone transversely fractured, just above the elbow joint, with a separation of its *condyles.*

"The fore-arm was bandaged from the points of the fingers up to the elbow. Now extension and counter-extension were applied to overcome muscular contraction, which was soon accomplished, and the fragments of bone were replaced in apposition; over which the roller was continued up to the shoulder and carried several times round the body to prevent its slipping down. Splints were also applied and the bandage carried back over them. Aperient medicines were prescribed and a low diet enjoined. My little patient never complained of much

* Transylvania Journal Vol. 1, p. 524.

pain, and recovered rapidly. At the end of four weeks he was considered well; but unfortunately for him, in about three weeks afterwards, he was climbing over a high fence, when he slipped, fell and broke his arm. I was again solicited to visit him. Upon my arrival I discovered the fragments of the bone disunited, and the soft parts greatly swollen. I treated the case as in the first instance. In two weeks re-union had taken place, and if I am not very much mistaken it is by *bone*. The child is now well and has the entire use of the arm.

Alleged causes of the milk sickness or sick stomach.—From a gentleman of age and observation, though not of the medical profession, who resides in the Western part of Kentucky, we are informed that 30 years ago some experiments were made, on the west fork of Red river, in Christian county and the adjoining parts of Tennessee, with the view of ascertaining the cause of this distressing affection; and that the result was the conviction on the minds of many of the inhabitants, that it was occasioned by cattle eating "the mass of vegetable matter growing in the bottoms of stagnant pools of water."

Another person, long a resident of a particular section of this State where the disease in question is frequently met with, seems positive in the belief that it arises from the cattle feeding on the *wild parsnep*. This name, however, is indefinitely given to several different vegetables and it is therefore difficult to say which particular one is meant—most probably an *Heracleum*, *Cicuta* or *Sium* is alluded to, since some of the species of each of those genera are known to be poisonous to horned cattle. Should this disease really be produced by our domestic animals feeding on a vegetable poison, (an opinion which at present we are disposed to adopt,) it is most probable that that poison will be detected in some one or more of the many noxious *umbelliferæ*, so abundant in the unsettled portions of the Western States. S.

Writ for the Medical Convention of 1830.—Whereas the Convention that was held at the City of Washington, in the month of January 1820, for forming a Pharmacopœia for our United States of America, did resolve that the President of that Convention should, on the 1st day of January 1828, issue Writs of Election to the several incorporated State Medical Societies, in the northern, middle, southern and western districts of the Nation, requiring them to ballot for three Delegates to a General Convention, to be held at Washington, on the 1st day of January 1830, for the purpose of revising the American Phar-

macopœia; and whereas the several Institutions, as aforesaid, are, by the same authority, requested to forward to the President, on or before the first day of April 1829, the names of the three persons so chosen; with sundry other provisions contained in the historical introduction to the work, to which the reader is referred.

Now therefore, I, SAMUEL L. MITCHEL, by virtue of the power vested in me, by the Convention of 1820, do hereby give notice, to all the incorporated Medical Societies, Colleges of Physicians and Surgeons, Medical Schools, and Faculties of Universities and Colleges, and all other authorized Bodies, that they choose proper persons to represent them in the General Convention to be held in January 1830, for revising the Pharmacopœia.

Given under my Hand, this first day of January, 1828, at the City of New-York.

SAMUEL L. MITCHEL, *President.*

PROSPECTUS OF THE

TRANSYLVANIAN, OR LEXINGTON LITERARY JOURNAL;

EDITED BY THOMAS J. MATTHEWS A. M.

Morrison Professor of Mathematics and Natural Philosophy in Transylvania University.

To diffuse more widely interesting and useful knowledge; to record the progress of discovery in the sciences and of invention in the arts; to mark improvements in the science and art of teaching; to advance the interests of education in all its departments, and to promote a pure and elevated morality, are the objects of "The Transylvanian." This work relies for patronage on those who have tasted the pleasures of knowledge, and whose patriotism prompts them to labour for its diffusion. Judicious efforts to increase intelligence in this Western country, which is soon to exert an important influence on the destinies of the whole Union, should be kindly cherished by every lover of freedom. Should this work receive the necessary encouragement, the editor, aided by several literary friends, will diligently exert himself to fill its pages in a manner satisfactory to its patrons. The profits of the work, after satisfying all pecuniary demands, shall be devoted exclusively by the publishing committee to the increase of the Library and Philosophical Apparatus of Transylvania University.

—

Terms.—The Transylvanian will be published monthly, in the octavo

form, each number containing forty pages of original and selected matter.

The paper and type will be similar to that of the Transylvania Journal of Medicine &c.

The price will be $2 50, due at the end of six months from the commencement of publication. To those who pay in advance a deduction of fifty cents will be made.

It is proposed to publish the first number in the month of January 1829.

Post masters and others who will obtain five subscribers and pay ten dollars in advance shall receive a sixth copy.

Communications for insertion, and letters on business of this journal should be addressed to "*Thomas J. Matthews, Editor of the Transylvanian, Lexington, Kentucky.*"

Lexington, Kentucky, January 1829.

OBITUARY.

Died on the 18th of December, 1828, at the residence of John L. Martin Esqr. in the neighbourhood of Lexington, George T. Ratrie M. D. aged about 30 years.

Dr. Ratrie was, we believe, a native of Maryland, having removed to the Western States a few years since, where he completed his medical studies, and graduated in the Transylvania School in the spring of 1824. Since that he had devoted himself chiefly to the practice of Dentistry, in which he had attained to very considerable and well-earned reputation. His success and respectability in life may well serve as an encouraging exemplar to junior brethren of the medical profession: in youth and till manhood he struggled hard with poverty and difficulties; but before he had reached its meridian, he was universally esteemed by those who knew him for his sterling worth and many estimable qualities; whilst his professional services were eagerly sought after by all within his reach.

To the numerous acquaintances and friends of Dr. R. it must afford a solace to learn, that he received, during a long illness, all that attention and kindness which devoted friendship and untiring attachment could bestow.

S.

Meteorological Journal, from October to December, 1828. By THOMAS J. MATTHEWS, A. M. Morrison Professor of Mathematics and Natural Philosophy, in Transylvania University.

Date.	Thermometer. Sunrise	3 P. M.	Sunset	Mean of Barometer.	Prevailing Winds.	General Remarks.
1	59°	62°	54°	28,872	variable.	Rain, cloudy.
2	41	60	52	28,978	ne.	Clear.
3	44	66	64	28,969	ne. & se.	Clear.
4	52	64	59	28,991	se.	Clear.
5	52	58	50	29,085	sw.	Rain, cloudy.
6	40	62	55	29,252	sw.	Clear.
7	42	68	58	29,328	sw.	Clear.
8	48	72	60	29,307	sw. & s.	Clear.
9	48	73	65	29,195	s. & se.	Clear.
10	53	74	66	29,058	sw.	Clear.
11	54	62	56	29,147	sw. & w.	Clear.
12	38	64	59	29,151	se.	Clear.
13	60	70	58	29,078	sw. & w.	Cloudy.
14	47	50	44	29,227	nw.	Cloudy, clear.
15	48	55	46	29,297	nw. & w.	Clear.
16	50	57	48	29,239	sw. & w.	Clear.
17	46	60	50	29,207	w. & sw.	Clear.
18	52	64	54	29,207	sw.	Clear, cloudy.
19	53	64	50	29,045	sw. & ne.	Cloudy, rain.
20	48	55	50	29,346	ne.	Cloudy.
21	47	60	54	29,462	ne.	Cloudy.
22	59	69	56	29,455	ne.	Clear.
23	58	67	55	29,439	calm.	Clear.
24	63	72	70	29,402	sw.	Clear, cloudy.
25	69	71	70	29,306	sw.	Clear, cloudy.
2	69	71	70	29,198	sw.	Rain.
27	69	72	67	29,033	sw. blus'g	Cloudy.
28	51	64	58	29,035	sw.	Clear.
29	47	68	48	29,161	east.	Clear.
30	57	66	61	29,133	east.	Clear.
31	60	66	60	29,092	east.	Cloudy.
	52	64°	57°	29.1837		

Mean height Thermometer, - - - - - 58 8-31°
Greatest height Thermometer, - - - - 73°
Least height Thermometer, - - - - - 38°
Mean height Barometer, - - - - 29.1837 inch.
Greatest height Barometer, - - - 29.462 inch.
Least height Barometer, - - - - 28.872 inch.

Date.	Thermometer. Sunr…	3 P. M.	Sunset.	Barometer.	Prevailing Winds.	General Remarks.
1	60	8°	62°	28,770	east.	Cloudy.
2	6	8	60	28,843	se.	Cloudy.
3	57	0	57	28,778	north.	Rain.
4	55	6	51	28,877	west.	Cloudy.
5	49	9	43	28,927	w. & n.	Cloudy.
6	32	2	51	29.121	west.	Cloudy.
7	51	52	51	28,967	south.	Rain.
8	38	51	48	29,232	nw.	Clear.
9	42	54	50	29,127	sw.	Clear.
10	45	3	56	29,025	sw.	Clear.
11	42	42	35	29,225	nw.	Snow.
12	30	45	40	29,360	calm.	M. clear, A. cloudy
13	39	44	40	29,298	se.	Rain.
14	38	40	39	29,122	ne.	Rain.
15	40	45	40	29,293	calm.	Rain.
16	39	44	41	29,173	calm.	Clear.
17	37	46	42	29,008	calm.	Cloudy.
18	44	51	52	28,784	se.	Clear.
19	57	52	44	28,658	sw.	Rain, blustering.
20	40	46	38	28,728	sw.	Cloudy.
21	36	43	40	28,917	east.	Cloudy, rain.
22	35	35	36	28,642	north.	Snow.
23	35	36	35	28,947	se.	Cloudy.
24	32	44	41	29,101	calm.	Clear.
25	32	50	41	29,343	calm.	Clear.
26	46	56	56	29,198	south.	Clear.
27	54	57	49	28,965	south.	Cloudy, rain.
28	47	52	43	28,973	sw.	Clear.
29	38	46	40	29,150	west.	Clear.
30	32	44	40	29,378	calm.	Clear.
	42	50	45	29.031		

Mean height of Thermometer, - - - 46°
Greatest height of Thermometer, - - - 70°
Least height of Thermometer, - - - 32°
Range of Thermometer, - - - 38
Mean height of Barometer, - - - 29.031 inch.
Greatest height of Barometer, - - 29.430 inch.
Least height of Barometer, - - - 28.595 inch.
Range of Barometer, - - - ,835 inch.

Date.	Thermometer. Sunrise	3 P. M.	Sunset.	Mean of Barometer.	Prevailing Winds.	General Remarks.
1	32°	50°	49°	29,420	se.	Clear.
2	50	55	53	29,305	south.	Clear.
3	58	67	54	29,092	se. & sw	Cloudy.
4	41	38	38	29,297	nw.	Cloudy, rain.
5	34	41	37	29,369	calm.	Clear.
6	38	41	32	29,237	calm.	Cloudy.
7	33	44	34	29,093	calm.	Cloudy.
8	32	44	43	29,157	calm.	Cloudy.
9	46	49	49	29,160	south.	Rain.
10	53	55	57	29,198	se.	Cloudy.
11	55	48	39	29,315	west.	Cloudy, rain.
12	28	46	42	29,212	calm.	Cloudy.
13	49	56	50	28,940	sw. & s.	Cloudy, rain.
14	32	40	36	29,130	sw. & s.	Cloudy.
15	27	42	35	29,235	sw.	Clear.
16	32	46	45	29,105	sw.	Clear.
17	39	43	35	29,187	se. & nw.	Rain, cloudy.
18	22	28	23	29,511	nw.	Clear.
19	20	40	29	29,439	calm.	Clear.
20	42	44	42	29,357	calm.	Cloudy.
21	40	44	38	29,322	nw.	Cloudy, clear.
22	22	34	32	29,190	calm.	Clear.
23	28	36	30	29,173	sw.	Clear.
24	28	36	30	29,183	sw.	Clear.
25	22	34	30	29,170	calm.	Clear.
26	20	36	34	29,238	west.	Clear.
27	42	56	50	29,190	calm.	Clear.
28	48	55	45	29,090	calm.	Cloudy.
29	46	55	53	29,060	sw.	Cloudy.
30	40	56	40	29,265	n. & sw.	Clear.
31	32	58	53	29,070	w. & se.	Clear, cloudy.
	36°	45°	40°	29,211		

Mean height of Thermometer, - - - 40°
Greatest height of Thermometer, - - - 67°
Least height of Thermometer, - - - 20°
Range of Thermometer, - - - - 47°
Mean height of Barometer, - - - 29,211 inch.
Greatest height of Barometer, - - 29,544 inch.
Least height of Barometer, - - - 28,845 inch.
Range of Barometer, - - - .699 inch.

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NOTICES.

1. Our subscribers and correspondents are requested to be more particular in addressing their letters and communications for the Journal, "TO THE EDITORS OF THE TRANSYLVANIA JOURNAL OF MEDICINE &c." This is the more necessary as there are one or two Literary Journals now about to commence in Lexington, and without care mistakes will arise.

2. The Essays on the Botany of this portion of Kentucky will be resumed in the next number of the Journal.

3. The May number will be published punctually on the first day of that month: consequently communications, intended for insertion in it, should be before the editors as early as possible. Some papers, offered for the present number, have been unavoidably excluded in consequence of reaching us too late.

4. Subscribers in arrears for the first volume are respectfully solicited to discharge them. If it be not convenient to remit $3 or 4, by mail, $5 may be sent, and the excess shall be considered as entitling the subscriber to a credit for advance payment for the present year, and he shall be so credited whenever the remaining $1 or $2 are paid.

Med Surgical Journa

The

Transylvania Journal of Medicine

and the Associate Sciences

Lexington, Ky.

Extra.

To go with

No. 1. Vol. 2. February, 1829.

*Transylvania Journal of Medicine and the Associate Sciences....*Extra.

I. Mr. W. P. NICHOLSON'S PAMPHLET,

AND

II. Dr. B. W. DUDLEY'S REPLY.

Dr. Short,

Dr. Sir, The enclosed manuscript was sent to me by its author with a request that I would present it to the editors of the Medical Journal for publication in their next number, or at as early a period as they can. Should the publication be deemed inadmissible in your paper, please return the manuscript to my son who will hand you this note.

I am with much respect your ob't serv't,

M. T. Scott.

Lexington, 28th January 1829.

Lexington, January 29th, 1829.

Dr. Sir,

The 5th number of the Transylvania Journal of Medicine (For February 1829) has already been published; consequentl[illegible] o communication, now offered, can appear in it. In reference to the manuscript, sent us for publication by Mr. Nicholson, and yesterday delivered to one of the editors with your note, our engagements have not yet afforded us an opportunity of examining it; but as soon as we have leisure to do so we will look over it, and then inform you whether we deem it admissible or not.

Respectfully &c. Your ob't serv'ts,

The Editors
of the Transylvania Journal of Medicine &c.

M. T. Scott, Esq.

Lexington, February 5, 1829.

Dr. Sir,

We have now taken an opportunity of examining the manuscript which you placed in our hands a few days ago, and find its contents to be essentially the same with that of a printed pamphlet, received a short time before through the post office.

As the matter of this pamphlet relates mainly to a personal difference which can in nowise "promote the science of medicine," we cannot allow it a place as a regular article in the Transylvania Journal. But inasmuch as Mr. Nicholson conceives himself aggrieved, through its pages, by the essay of Dr. Dudley, and since he has chosen to address his publication to us, that all manner of justice may be done him, we have determined to reprint his communication in an extra form. This will be accompanied by an answer from Dr. Dudley, and together they shall be sent to all our subscribers, to the end that they and the public at large may be enabled to judge fairly of the points at issue between them.

Respectfully &c. yours,

The Editors
of the Transylvania Journal of Medicine &c.

M. T. Scott, Esq.

I. Mr. W. P. NICHOLSON'S PAMPHLET.

BOTH SIDES.

Extract from the Transylvania Journal of Medicine and the Associate Sciences, Vol. I. No. 14,* *November* 1828—*page* 532.

DUDLEY ON THE BANDAGE.

"It is rather a singular fact, that on a subsequent occasion, another individual, who had been under my charge, for an injury done the head of the fibula, some time after leaving me, became the patient of the professional character, who had failed in the treatment of the case just recited; and when this very simple case eventuated in the loss of the limb, the patient was taught to ascribe that loss to his treatment, while under my charge.

The history of the case is as follows. In attempting to mount his horse, the patient was thrown with the inferior and outer part of the knee on a piece of timber. In a few minutes I was called, and upon examination, the head of the fibula gave to my hand the sensation of an egg-shell crushing between the fingers; no other part of the limb received the slightest injury. The patient was in great pain, which was attributed in part to the injnry sustained by the peroneal nerve. The bandage was applied, extending from the toes until it passed the knee, notwithstanding a variety of imprudences, committed by the patient, in rolling in bed, in eating and drinking secretly, articles of the most injurious kind, whereby two severe attacks of fever were produced, and in interrupting the dressings; yet about the twenty-second day from the injury, he was well enough to leave me and go to the country.

He was not long in the country, before I was informed that he was again ill, and that his leg was swelled and painful near the ankle. The individual who could lend the weight of his word to propagate error and do injustice, in a case, the history of which affords sufficient evidence in favour of the treatment instituted, only exposes his own weakness and folly to the derision of the intelligent in the profession.

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* In the printed copy of Mr. Nicholson's pamphlet directed to the "Editor of the Transylvania Journal of Medicine, &c. Lex. Ky." we find this note in manuscript,—"The Ed. will please return the MS. to Mr. Scott and insert from this. W. P. N." Consequently we follow the printed pamphlet literally, although it differs in some respects from the MS. copy, and contains some errors in the extract from our Journal.—Eds.

Had the bandage been judiciously continued after leaving me, (*I will here remark that the bandage was continued until the day my limb was amputated; probably not with that* JUDICIOUS *power in which it was first applied,*) the patient might have been the subject of fever for months in succession, from improper indulgence in eating and drinking, without necessarily losing his limb. Nothing can exceed the folly and stupidity of the charge against the use of the bandage in that case; the more especially as the patient was well enough after two attacks of fever, induced by imprudence, and the injury done his knee and the head of the fibula, to leave town in twenty-two days. It must be manifest even with the tyro, that the evil effects of the bandage, when they do occur, are speedily made known. Swelling, inflammation, deep seated abscesses, suppuration and sloughing, are incompatable with its direct action on any part. Its tendency is immediately opposed to each of these states of the system. The law whereby constitutional diseases are inclined to spend their force on parts rendered prematurely weak and excitable by violence or otherwise, seems to have escaped attention.

It is from that law, a solution of the case in question, is to be drawn; while from ignorance or neglect of it, the patient ultimately lost his limb. By means of the bandage, I have been enabled to save more limbs, the removal of which had been determined on by other physicians, than all the cases of amputation together would amount to, in which I had the least agency, either immediately or otherwise, for the last fourteen years.

To the Editor of the Transylvania Journal of Medicine and the Associate Sciences.

SIR—On perusing one of the late numbers of your Journal, I perceived several statements of cases therein, intended to illustrate the utility of tight bandaging, under the signature of Dr. B. W. Dudley, the professor of Surgery in the Transylvania University.

The case which particularly arrested my attention, is the one that he most fully dwells on, in the 532 page of the 14th number, for Nov. 1828, which is evidently intended as a representation of my own case. Presuming that your Journal was established to promote the science of medicine, to impart correct information to the student, and to enlighten the community at large, by publishing to the world facts, and correcting or exposing *errors*, I have therefore deemed it necessary to address you, in answer to Dr. Dudley's statement of my case as alluded to above. Had he confined himself to the facts it actually presented, without descending to the gratuitous task of commenting upon my habits at the expense of truth, in violation of his duty as a professional man, and at the sacrifice of the ordinary courtesies of life, I should not, at this late day, have trespassed

upon your attention, or claimed the public ear. But, as he has thought it to accord with his principles as a gentleman, and reputation as a surgeon, to add *insult* to the injury he has already inflicted upon me, through his culpable ignorance of the nature of the injury I had received, and want of judgment in the treatment of my case, and seems disposed to exculpate himself from censure, by the heralding forth his good fame, at the expense of my reputation, as well as that of others, justice requires that I should arrest the current of erroneous impressions, which it is too evidently the wish of Dr. Dudley to impose upon the public. Were his statements destined to be seen alone by those to whom we are both known, or by those who could have an opportunity of informing themselves rightly as to facts, I should have treated his production with that contempt, which, from its malignity and falsehood, it so richly merits; but as such is not the case, I must be indulged while I rescue my character from the insidious stab of one, who avails himself of official station, to injure an individual whom he, by his want of skill, has rendered a cripple for life.

To begin then with Dr. Dudley and his statement. I am really at a loss to conjecture to whom he alludes when he says, that "I was taught to ascribe the loss of my leg to *his* treatment, by a professional character, into whose hands I fell subsequently to being under his charge." In his published history of my case, he states, that "in attempting to mount his horse, the patient was thrown on a piece of *timber*." Now the fact is, that in attempting to mount the horse, my cloak caught, and I could not gain the stirrup, and in jumping off I sprained my leg, as will appear in the sequel. I fell on a grass plat; but as to the piece of *timber* the Doctor alludes to, if there, it must have been buried under ground, as it was neither visible to the sight, nor tangible to the touch. His notice is the first intimation I ever had of it. He then says, that "upon examination of the head of the *fibula*, it gave to his hand the sensation of an egg-shell crushing beneath his fingers; no other part of the limb received the slightest injury." It certainly cannot have escaped the memory of Dr. Dudley, as well as that of several other persons, who were present when I met with the accident, that not more than ten minutes elapsed before he was present, and that after pulling off a tight boot, and examining the limb, he observed it was only a sprain; but upon a more close examination, he said both bones were broken about three inches below the knee, and employed two gentlemen in holding the broken bones, while he was preparing the bandages, which he applied in the following manner: he first tore off five or six small bandages about two and a half inches in width, and about two feet long; these he saturated with whiskey, and bound over the knee; he next applied the long roll bandage, (which was also saturated in spirits,) beginning at the toes and extending upwards nearly to the hip-joint. Being thus bandaged up like a harlequin and placed upon a bed, but a very short time had elapsed before I felt the most excruciating tortures, and observed to those present, I could never stand it; that the pain created by the blood endeavouring to force its passage to the extremities of the injured leg,

was beyond endurance. I felt the pain just above the knee joint, and in the tendons underneath the knee.

My friends taking it for granted that the Doctor ought to know best, notwithstanding the remonstrance of the Rev. George C. Light, who wished to relieve me, suffered me to remain in the situation I have described, until I became delirious, under the baneful effects of the tight bandaging. While in this situation, I may have been guilty of the imprudence of rolling in bed, as reported by Dr. D.; but agreeable to the representation of my friends, Dr. D. was sent for three different times during the first night; the first time he sent a message to this purport—that I must endeavour to bear the bandages, and that they ought not to be removed (I think) for two weeks. To the second message he returned a note, stating that he would not have the bandages removed for one hundred dollars, and that the cause of my extreme pain arose from the circumstance of the fracture extending into the knee joint, and that he could compare it to nothing else than egg-shells beat in a mortar. The third time he was sent for, he came, and observed, he was astonished I had no more philosophy. Philosophy! Philosophy in a man partially deranged by tight bandaging, which prevented his blood from circulating? Had Job, with all his patient forbearance, been driven to madness by the empericism of his physician, I question very much whether his philosophy would have availed him much more than mine did me. In the morning of the second day, when he came to see me, Dr. D. loosened the bandage to the knee joint, and, if I mistake not, it was relieved again the same day by Dr. Webb, by the consent of Dr. D. The bandaging from the knee to the extremity of the foot, was not removed for several days, and always reapplied as soon as wet with spirits, as before. In this situation I was kept for twenty-six days, (Dr. D. says twenty-two.) Whether the variety of imprudences as ascribed to me by Dr. D., such as rolling in bed, &c. arose during the first forty-eight hours, when labouring under delirium, brought on by his want of judgment, I know not; it is, however, a pitiful subterfuge for him to take shelter under such a flimsy covering, from the well deserved censure, consequent on his ignorant treatment of me. The next items in his list of my imprudences, are his charges against me of *eating* and *drinking secretly*, articles of the most injurious kind. This is evidently an afterthought, eminently worthy of one so skilled in surgery. Is it not too ridiculous for serious refutation, that an invalid, who was unable to move from off his bed, could procure articles of food and drink *secretly*? But if they were so secretly disposed of, how came the fact in the possession of Dr. Dudley? Did he discover a knowledge of it by the same penetrating eyes that enabled him to discern the *timber*, on which his *imagination* so ingeniously broke my leg? He certainly should have explained the name, and quality of those injurious articles, in which I indulged, as also how and by whom he obtained his information. By whom was I furnished with them? wherefore the necessity for secresy? I was not in a situation to demand of me a resort to any such means, to gratify my appetite. I had arrived at the age of maturity, and was sur-

rounded by those, whose hospitality, kindness and attention were only equalled by their sympathy for my sufferings, and solicitude for my relief—whose indulgence would have precluded the necessity of resorting to secresy, to procure either the necessaries or luxuries I might have needed or desired. Instead of its being necessary to practice covert devices to procure articles of "a pernicious kind" to indulge in, it was not an unfrequent, though a painful duty I had to perform in refusing the numerous litttle messes provided by the generous thoughtfulness of those, by whom it was my good fortune to be surrounded. I say the duty was a painful one—it was truly so to me, to be compelled by a sense of propriety, in obedience to the directions of my surgeon, to reject the proffered kindnesses of those whose greatest pleasure it was

"To bind up the wounded heart,
And pillow the aching head."

It is well known that I neither ate nor drank any thing out of the common prescriptions given by Dr. Dudley, for my guidance, without consulting him, except in one or two instances, which I shall notice, during the whole course of my confinement, while under his charge. I recollect a few days after the accident occurred, of asking for a glass of *wine sangaree*, which was handed me, though *not secretly*. I had scarcely tasted it before Dr. Dudley came in, and pointing to it, I asked him whether it was proper for me to drink it; and on his replying that it was not, I desisted from doing so. Again, about eight or ten days from this period, I drank about a tumbler of cider without his consent; but this was done openly and not *secretly*. About the third week after I met with my accident, and after Dr. Dudley had pronounced that I would *soon* be well, (although my leg was then as scaly as the back of a fish, and smelt, as I was told, very offensively,) I did, as he states, perhaps imprudently, eat a few *oysters*. These are the only times that I recollect of having either ate or drank any thing out of the range of Dr. Dudley's general prescriptions. It is perfectly within my remembrance, that after I had eaten those oysters of which I have just spoken, upon Dr. Dudley's being informed of it, that he said they were the most injurious things I could have eaten, and that a person present observed to him, "Doctor, when I was so ill, you recommended them to me." "Yes," was his reply, "but those that I recommended to you, *were cured in their own liquor*." He was then informed that those I had eaten were also cured in their own liquor, and not with vinegar. After this explanation, he admitted they were innocent. Notwithstanding this admission, in his desperation, like a drowning man catching at straws, he has continued to harp upon this oyster business. I feel satisfied, after this candid detail of facts, that the intelligent part of the community will conclude, that the reason assigned by Dr. Dudley as the cause of his failure, is no less impotent than unjust, and but serves to expose "his weakness and folly to the derision of the enlightened in the profession." I recollect but once of having any considerable increase of fever, except what was induced by the cause already assigned, while under Dr. Dudley's care, when at my request he gave me a dose of medicine. This was some

time after the "*egg-shell fracture*" took place. I am the more certain of this circumstance, because of my having asked him if the medicine was not *calomel*, and observing to him that I was in the habit of taking from fifteen to eighteen grains. His reply is distinctly within my recollection; it was, "if ten grains would not have the desired effect, fifty would not."

I shall now notice the journey I performed, which, as the Doctor says, was about the 22d day, but which, in fact, was on the 26th day. He says, "nothing can exceed the folly and stupidity of the charge against the use of the bandage in *my* case, the more especially as the patient was well enough after two attacks of fever induced by imprudence, and the injury done his knee, and the head of *his fibula*, to leave town in twenty-two days." Here the Doctor is at points with himself. He certainly must have forgotten that in giving a history of the case, at the commencement, he says, no other part of the limb received the slightest injury except the *head of the fibula*, which was like the crash of egg-shells; but when he recites my journey, performed on the 22d day, in order to impress the public with the idea that the case was a very desperate one, (which, unfortunately for me, was made so by his mismanagement,) and that consequently the case was bordering on the miraculous, he recollected of a sudden that the knee was injured.

Now in all human probability, if the Doctor had written another paragraph, he might have recollected the circumstance of his giving two gentlemen the trouble of holding *two bones*, which he represented as being broken, whilst he was preparing and applying the bandages. It is unfortunate for the success of the Doctor, that he is not able to continue this delusion, as with the aid of those two bones, which were never broken, except in his imagination, he might be able to make out a tolerably specious case. Facts, however, stand too strongly arrayed against him, and he has been compelled to yield to circumstances. Owing to his wretched misapprehension of the nature and extent of the injury I had received, and barbarous application of the bandages, the flesh sloughed off, and exposed the bones to view, when it was discovered that there was not the slightest crack perceptible—no evidence of *broken bones*.

This can be established by some of the most eminent physicians, as well as by several respectable gentlemen in and around Lexington. Thus driven to the wall, the Doctor has very prudently dropt the *broken bones*, conscious that so flagrant a mistake would be received as a proof of his defective judgment, and as a reflection upon his skill as a surgeon.

A more gross misconception of the character of an injury, I think I might venture to assert, is not to be found in the books; nor never did any individual suffer more through stupidity than I have done; for, by what other name shall I designate that condition of the mind, which mistakes a simple sprain, for an injury of the complicated kind, as ascribed to mine by Dr. Dudley? I will return to my case. The day succeeding the one on which I met with the accident, Dr. Webb, as before premised, came to see me, and observed that if the bandage remained in

its then situation much longer, I must either lose my limb or my life; and if I mistake not, it was at his suggestion the bandage was loosened the first time to the knee, by Dr. Dudley. About the second week, my friends, finding my situation growing worse, called in another physician, Dr. Pindell, in consultation with Dr. Dudley, to whom Dr. D. represented the "*broken bones*" and the "*egg-shell fracture*," observing to him that he should not like to remove the bandages. Dr. Pindell did not insist upon their being removed, but replied that he should have preferred putting my leg in a box, and advised the application of warm vinegar, which was poured over the bandages, with the approbation, as I presume, of Dr. Dudley. After this, Dr. P. made two different appointments to meet him in consultation, but Dr. D. both times failed to make his appearance, which occasioned Dr. P. to desist from visiting me, and I never saw him again until after my removal from Lexington.

After a day or two, Dr. Dudley stopped the application of the vinegar, observing it was of no use; that the application of the bandages was all that was necessary; and that the reason why most of the physicians were so averse to using them, was because they did not know how to apply them. That they were like a set of old dray horses, (I think he said,) that could not be got out of the old beaten track. If I am not egregiously mistaken, he also stated that the old fashioned mode of using *splints* in cases of fracture was entirely abandoned, being superseded by the application of the bandage, by the first surgeons in Europe.

My limb, in a very short time after this, became very offensive, and Dr. Dudley will recollect, he was asked the cause. What he attributed it to, I was not informed until some time after my removal to the country, whither he was not invited, his services not being desirable. It must, I think, be obvious to all, that mortified pride has driven him to the unenviable task of accounting by means of slander and falsehood, for consequences arising from his own mal-treatment of me, and hence springs his charges of the secret use of articles of a pernicious tendency, which he has so uncharitably trumpeted forth to the public against me.

It may be proper to remark in this place, that whilst under the direction of Drs. Pindell, Satterwhite and Webb, my course of diet was altered, and instead of restricting me, they gave me stomachics to increase my appetite. This they deemed necessary to counteract the ill effects which were likely to result to my general system from the copious discharges of my leg, it being their opinion that if my appetite failed, I could not survive many days. I would here ask, is it probable that a little *wine sangaree, a tumbler of cider, and a few oysters*, used during a period of twenty-six days, could have produced such unhappy results? If so, what could have produced that scrofula appearance and purple spots, before I either drank the cider or tasted the oysters. Surely it was not barely taking a mouthful of wine sangaree to wet my lips, and moisten a parched throat. No, the cause is to be traced to the application of the bandage, and to that alone, notwithstanding "the law whereby constitutional diseases seem to have escaped attention, and the four-

teen years experience" (of Dr. Dudley,) in the application of tight bandages.

But to proceed; knowing I was trespassing upon the generous hospitality where I was then unavoidably confined, I was anxious, as well as my more interested friends, to remove to the country, and Dr. Dudley must surely recollect being questioned as to the propriety of the measure; if he does not, I perfectly remember his answer, which was, that I had better remain a few days longer, that I would *soon be well enough.* I waited patiently several days, in hopes *well enough* would arrive; but instead of getting better, I evidently grew worse. Dr. Webb at last insisted upon my removal, stating he could not, with propriety, interfere while I remained under Dr. Dudley's charge; and without consulting him, I was removed to the distance of two and a half miles from town. Dr. Dudley would fain have the public believe that when removed to the country, my leg was nearly well, than which nothing can be farther from the fact. I will describe the manner of my removal, from which it will be perceived that I was as low as I well could be. Dr. Webb removed me in a carriage, on the bottom of which a bed was placed. I was taken from the chamber in which I was confined, and carried to the carriage, and carefully laid on the bed provided for my reception. Thus situated, I was conveyed by slow travelling to the residence of M. T. Scott, Esqr. lifted out of the carriage, and carried to a bed previously prepared for me.

So much for Dr. Dudley's Flemish account of my being well enough about the twenty-second day, to leave him and go to the country. The day after my removal, after I had somewhat recovered from the fatigue of this *wonderful* journey, Dr. Webb removed the bandages, when it was discovered that there were several purple spots, and one black one, about three or four inches below the knee, on the outer part of the leg, from which a copious stream of the most offensive matter immediately issued, nearly filling a quart vessel. As soon as it had stopped running, Dr. Webb went to Lexington for Dr. Pindell, who again attended me as a consulting physician. In a short period after this, every muscle in my leg began to decay, and finally rotted off; my heel and part of the toes sloughed off. It was the opinion of the respectable physicians who attended me, as well as that of other intelligent persons who witnessed my sufferings, that the whole was ascribable to the improper application of the bandages, and *that* during the first forty-eight hours.

In all probability, had the sprain, for it was nothing more, been left to nature, she would have performed a cure in a few weeks; but it was my misfortune to call to my aid a votary of tight bandages, who, to gratify an unhallowed pride of opinion, has compelled me to wear a wooden leg "*as a monument of bad surgery.*" The fact is, that if Dr. Dudley was to put the same pressure upon an uninjured leg, that he did upon my sprained one, the same disastrous results would no doubt be produced.

Again, Dr Dudley attributes my sufferings and loss of limb to the interruptions of the bandages or dressings while under his care. This

is the more strange, as he acknowledged to M. T. Scott, Esq. that during that time, the bandages were never removed but by himself, except once partially, and then by his directions. The Doctor is not, however, over scrupulous in framing excuses; for upon another occasion, in despite of the facts I have detailed, he asserted that if I had remained three days longer under his care, I would have got well. If there had been the slighest ground for such an assertion, how is it possible that the inflammation, deep seated abscesses, suppuration, and sloughing, that *immediately* followed my translation from Lexington, could have taken place. It will not be contended that this instantaneous change is attributable to the effects of the country air; if not, what was the cause? Nothing more nor less than Dr. Dudley's tight bandages. The excuse which I have just exposed, calls to my mind a conversation which he had with the same gentleman, in reference to the application of his bandages. He gravely contended that if he had the strengh sufficient to exert a force equal to the power of ten horses in their application, it would be impossible to injure a limb, provided the bandages were wrapped regularly. I merely mention this to convince the public how carefully the Doctor is to outrage decency and common sense, rather than his favorite hobby should not be sustained. Every one who knows any thing about the matter, must know that such a force applied to a limb, would grind it in good earnest to an *egg-shell* substance; it would be better for the Doctor to be candid for once, plead guilty, and abandon his leg-destroying bandages; for unless he does, should he have many cases, he will have much to answer for; for each one will have cause to bear testimony against him. The Doctor really seems to have set out with a determination to run a tilt after dilemmas, as will appear from the following. He says, "He (I) was not long in the country before I (he) was informed that he was again ill, and that his (my) leg was swelled and painful near the ankle."

Now it so happened that my illness had not intermitted. But why should he particularise the pain as being "*near* the ankle?" The fact is, there were many other parts of the leg which caused me infinitely more suffering and pain. To me it seems somewhat singular, that the Doctor's information should be so precise at this remote period as to enable him to locate the *seat* of *pain;* for on reference to a note written by him only 11 months after the accident occurred, he affects to have forgotten both me and my case.

Notwithstanding all Dr. Dudley has said to the contrary, it is an undeniable fact, which can be substantiated by several distinguished gentlemen of the faculty, that my leg was in a state of mortification when my friends removed me from under his care, extending from 4 or 5 inches above the knee to the end of my toes. To the skill of the physicians who attended me, after my removal, together with the unceasing attention of my friends, am I indebted for my recovery. To Dr. Webb, who scarcely ever left my bed side during a period of five months; to Drs. Pindell and Satterwhite, (the two latter attended in consultation,) I owe a debt of gratitude, I fear I never shall repay.

In about five months after my accident, my situation being considered extremely precarious, Dr. Scott of Chillicothe, (now of Lexington) was written for; but having unfortunately met with an accident the day before his intended departure with a view of visiting me, he was prevented from coming, and I of availing myself of his services at the time.

In the month of June following, Dr. Webb being anxious of returning to Ohio, by the advice of Dr. Scott, and the approbation of Dr. Pindell, who were consulted upon the occasion, I was removed to Chillicothe, a distance of 150 miles. It was considered by my physicians, that in my low and doubtful condition, a change of air might be serviceable to me. At the period of which I am now speaking, five months after the accident, notwithstanding the Doctor, in his report, alleges I was "*well enough*" to leave him and *go* to the country about the 22d day, I was so reduced that my back and hip bones had come through the skin, and I was wholly unable to help myself. I was so low that it was found necessary to fix the bed on which I was laid, on a springy board in the carriage; I was lifted in and out as occasion required. Mrs. N. and a servant, besides the driver, accompanied me in the carriage, to render such assistance as might be requisite, and Dr. Webb and his brother accompanied on horseback. On my arrival at Chillicothe, Dr. Scott examined my leg, and then pronounced it as his opinion, that it would never be of much service to me; but that after it should have been healed, I would be the better able to judge, and if it should be necessary then to have it taken off, I would be in a better situation to undergo the operation. In about four weeks from my arrival in Chillicothe, I had so far recovered as to be able to set up, and occasionally to walk with the aid of crutches, and a stirrup suspended from my neck, so as to support my skeleton of a leg. In the month of August following, by the advice of Dr. Scott, I returned to Kentucky to avoid the autumnal fever which had prevailed for one or two years in Chillicothe.

In the month of November, I had so far regained my strength, as to be able to leave Kentucky for Baltimore, my leg being supported on a pillow in the carriage.

Thus after *eleven* months of the most intense suffering, I was enabled once more to reach my home, with a mutilated limb, produced from the want of skill and judgment in my medical attendant at the commencement of the case.

On my arrival in Baltimore, Dr. Buckler, who is my family physician, called to see me, and on examining my leg, said it did not *appear* that any bones had been broken, but that it would never be of any use to me, and advised amputation; at this period my leg was nothing but skin and bone, there being nothing left but the artery to nourish it, the little flesh and muscles it used to have, having rotted off.

At Dr. Buckler's request, Drs. Davidge and Jamison, were called on in consultation. On examination, the latter gentleman coincided in opinion with Dr. B. and recommended amputation; the former agreed as to the first point, but recommended amputation to be deferred for a time to see the effect the efforts of nature might produce. Desirous, if

possible, of saving my limb, I awaited the operations of nature until the 24th of April following, when despairing of recovery, on that day the amputation was performed by Dr. Buckler and Dr. Gibson, and in the presence of Dr. Lyon, now demonstrator of anatomy in the medical college of this city, Dr. Keerl who is now in Europe pursuing his studies, and Drs. Coskery, Caldwell, Pearce and Handy; those gentlemen, who dissected and examined the limb after it had been taken off, told me there was no sign of fracture that they could discover. Thus ends the history of the case of the "broken bones," the "egg-shell crash, and the injured knee," after seventeen months of the most excruciating torture. It is unreasonable to presume for a moment, that effects so extraordinary could have proceeded from a simple sprain, unless superinduced by improper treatment. To Dr. Dudley's want of judgment and tight bandages, I am indebted for all my suffering and the loss of my limb.

In about six weeks after the operation was performed, I walked to my office, distance probably a quarter of a mile—so much for a skilful surgeon. In the month of November following, I mounted a wooden leg, and have visited Kentucky three times since then; during the last summer and fall, I spent the most of my time in and around Lexington, and never heard a word of my case, until the day previous to my leaving Lexington, when I was informed it was preparing for the press. A few days after my return here from Kentucky, I received a copy of his publication through the medium of the post office. To me it is inexplicable what fatuity could have beset Dr. Dudley, after an elapse of *four years*, to publish my case; much less can I account for the worse than savage pleasure he seems to indulge in, while unprovokedly assailing me. Had Dr. Dudley stopped after giving *his imaginary* statement of my case, erroneous as that was, I should have permitted him to have enjoyed his laurels undisturbed; but as he has calumniated me, and called my morals in question, I have felt myself bound to expose the grossness of his professional errors, and to throw back the foul aspersions upon my reputation, as unworthy of me, and becoming only him, in whose malevolent heart it was nurtured. In a word, I defy him and the miserable beings whom he has employed, or those who gratuitously lent their aid, to propagate the charge of eating and drinking to excess against me, to substantiate it, by the evidence of a solitary individual entitled to credit in the estimation of honourable men.

In conclusion, I will appeal to the public, and ask them, admitting for argument sake, that Dr. Dudley's representation of my case was correct, which I deny—I say I ask them, I ask the intelligent of the medical profession, whether under the circumstances of the case he has made out, he was justified in the application of tight wet bandages? I ask them did they ever see, hear, or read of such treatment, except by Dr. Benjamin W. Dudley, in a case of *egg-shell fracture*, in Europe, Asia, Africa or America? And whether it succeeded? I have done with the Doctor.

I am the public's ob't serv't,
W. P. NICHOLSON.

CERTIFICATES.

The following is in answer to a letter I addressed to Dr. Buckler some time after my limb had been amputated.

COPY.

Dear Sir—In answer to your note of the 24th, in which you desire to know "if I could discover any appearance of fracture in the bones of your amputated limb, more particularly of the head of the fibula and knee cap," I state, for your satisfaction, that no trace of such injury was discoverable in either of these bones, nor could I perceive any sign by which I could infer that the larger bone of the leg or tibia had been fractured.

Respectfully, &c.

May, 30*th*, 1826. J. BUCKLER.

My object in procuring the annexed certificates is merely to show that Dr. Dudley was entirely mistaken in the nature or extent of the injury I received.

We, the undersigned, were present at the amputation of Mr. Nicholson's limb, and have again examined it this day, and do not perceive any signs of fracture. Signed,

S. H. LYON, M. D.
S. H. CALDWELL, M. D.
W. D. COSKERY, M. D.

WITNESS, W. A. SCHÆFFER.
Baltimore, Jan. 9th, 1829.

We have examined the bones of a leg, handed to us by Mr. Nicholson, and do not perceive any evidence of fracture in either of them; and the head of the fibula appears perfect.

Signed, GEORGE S. GIBSON, M. D.
MAXWELL McDOWELL, M. D.
MICHAEL DIFFENDERFFER, M. D.
JOSHUA I. COHEN, M. D.
GEORGE FRICK, M. D.
WILLIAM DONALDSON, M. D.

We have examined the bones of a leg, which was handed to us by Mr. Nicholson, and particularly the head of the fibula, and do not perceive any evidence of fracture.

Signed, JOHN J. McKENZIE, M. D.
S. BIRCKHEAD, M. D.
ALEXANDER CLENDINEN, M. D.
ASHTON ALEXANDER, M. D.
SAMUEL BAKER, M. D.
W. FISCHER, M. D.

I do hereby certify, that I buried Mr. Nicholson's limb, and at his request took it up again the other day, cleaned it, and gave it to him, and that the limb now before me is the same one.

Witness my hand, this 9th of Jan. 1829.

JOHN C. BROWN, *Sexton of Christ Church.*

State of Maryland, City of Baltimore, sc.

On the 9th of January, 1829, before me, the subscriber, a justice of the peace for the said city, personally appeared John C. Brown, Sexton of Christ Church, in said city, and made oath on the Holy Evangely of Almighty God, that the several facts contained and set forth in the above certificate, are respectively true as they therein stand stated.

Sworn and subscribed before me the day and year first written.

W. A. SCHÆFFER.

I certifiy that I was present when John C. Brown buried Mr. Nicholson's limb, and also when he took it up again, and that the limb now exhibited is the same one.

WITNESS, W. A. SCHÆFFER. W. T. PEACHEY.

Baltimore, 9th January, 1829.

"I was called, and upon examination, the head of the fibula gave to my hand the sensation of an egg-shell crushing between the fingers. No other part of the limb received the slightest injury."

Dr. Dudley's Statement.

II. Dr. B. W. DUDLEY'S REPLY.

A man who has lost a limb through folly is sufficiently punished for his indiscretion; and the public, in the exercise of charity, turns an indulgent ear, when an effort is made to prove that he is not the author of his own misfortune.

Mr. Nicholson is given to understand that I feel no disposition to render his case worse than it really is; on the contrary, I am quite willing to concede to him all the benefit of the argument, that he was in a state of derangement not only for the first two days, as stated by himself, but that he remained so during the whole period of his confinement under my charge. In this concession there is quite as much charity shown to Mr. N. as there is justice done those who have urged him to the commission of an outrage upon truth and virtue in his slanderous publication. Had the same pains been taken to conciliate, that have been to poison his feelings against me, Mr. Nicholson might now be numbered among my friends. Reasons, however, existed among those who directed his opinions and fixed his prejudices, for turning both against me.

Not many weeks after he was withdrawn from my charge and was carried to the country, there were so many falsehoods and slanders circulated to my injury, that I wrote a letter to M. T. Scott, one of the brothers-in-law at whose house Mr. Nicholson then was, and threatened to lay the whole case before the public. By reading the pamphlet of Mr. Nicholson, I am possessed of additional and satisfactory reasons for believing, that his brothers-in-law committed the first outrage on correct behaviour, and that to their interference he is especially indebted for his irreparable misfortune; since according to his own admission, his brother-in-law, Dr. Webb, (who at that time was scarcely familiarised with the initials of the profession, and who could not approach me in the light of a physician,) told him only the day after the injury, "that if the bandage remained in its then situation much longer, I must either lose my limb or my life." From that time the series of imprudences commenced, as well in relation to improper meddling with the limb, as to indulgences of appetite.

During the absence of Mr. Nicholson from this state, and while under the charge of another brother-in law, Dr. Scott, then of Chillicothe, Mr. M. T. Scott procured of me my bill against Mr. Nicholson, with

the intention, as I thought, of paying it off. But in the October following, after the return of Mr. Nicholson from under the charge of Dr. Scott, he addressed me the following note.

"To Dr. B. W. Dudley: Sir, I propose leaving this country in the course of two or three weeks for the eastward, Baltimore, if my situation permits. As it is not my intention to pay you unless compelled by law I have thought it thus proper to inform you of my contemplated journey in order that you may take such steps as may be deemed most adviseable. Your serv't. WM. P. NICHOLSON."

To this note I returned the following answer:

"Mr. W. P. Nicholson:

Sir, I received your note this morning in which you apprise me of your determination to go to the eastward, and that you will not pay my bill which your brother-in-law (M. T. Scott) got of me some six months ago, unless compelled by law.

A legal process before one of our justices of the peace, wherein a Kentucky doctor is plaintiff and a Baltimore merchant defendant, would be truly a novel scene. There are two orders in society against whom I never prefer a claim for professional services, and you are certainly at liberty to rank yourself in my estimation, with either of these. Since you have chosen to recall your case to my recollection in the uncivil style of your letter, I have now to add, that I apprehend I pity your folly as much, as you can love your money, or lament your misfortune.

Respectfully, B. W. DUDLEY."

What agency the brothers-in-law of Chillicothe had in causing Mr. Nicholson to write me the foregoing note, immediately after his return from that place, the history of the case will tell. But inasmuch as Dr. Scott could not have forgotten a case of fractured leg under his charge, followed after some time by fistulous abscesses, in which he could not effect a cure; and since he must be apprized that I succeeded in that case in a short time by the use of the bandage, I had some claims on *him* to become the advocate of the practice, and to reconcile his brother-in-law, Mr. Nicholson, to what I had done.

Mr. M. T. Scott has played a part in this business not at all calculated to show his love for consistency, or his regard for disinterested justice. Regrets expressed to one gentleman at the injurious reports in circulation, while currency is given them when in conversation with another; threats or surmises of personal danger, prostration of character, law-suits, and answers in print to my notice of his brother-in-law's case in the journal, are held out at one time; while at others it appears he would like to approach me as a friend and beg me to desist, except that he was forbidden by some unknown cause of anger on my part. Mr. Scott cannot be ignorant of the source of my humble opinion of him.

But I have other reasons for believing that Mr. Nicholson has been goaded on by the bad feelings of his friends in Lexington to make the late unwarrantable attack upon me. Although Mr. Nicholson speaks

of having perused the number of the Journal containing my communication at which he has taken such offence, I do not believe he has ever seen that number, and that he has based his pamphlet on an incorrect manuscript copy of that part of it, relating to his own case only. Otherwise I cannot conceive why he should have addressed his publication to "the *Editor*" of the Transylvania Journal, when it bore on its title-page the names of two gentlemen as *Editors;* how he could have mistaken 4 for 14 in allusion to the number of that work from which the extract is taken; how he could have copied *prematurely* for *preternaturally; immediately* for *immediate;* how he could have thrown two sentences into one, by placing a comma where he found a full point; or have made detached paragraphs in his copy, where there were none in my essay; thus perverting the meaning and making nonsense of it. These errors do not savour of the mercantile accuracy of a man of business, and it is quite certain that no printer would have made them from a printed copy. They speak for themselves.

Mr. Nicholson himself complains, after so long a period has elapsed since he sustained the injury, that I should now unfeelingly throw his case into print, and attempt to injure his moral character. Thus far Mr. Nicholson has enjoyed all the privileges of a one-sided story, told by himself and friends. From Baltimore, Philadelphia, and New-York, enquiries have been made of me concerning his famous case. Even now I would suffer him to enjoy all the benefit of his pamphlet, and would be silent, except for the array of professional names, under the weight of whose authority, he no doubt expects to repose in quiet, and to enjoy the satisfaction of witnessing my humiliation. This circumstance gives his communication an importance in the public eye which it otherwise would not possess, and seems to demand from me some observations in reply. I therefore proceed to a plain statement of facts and a few remarks thereon.

Besides the various articles of drink and diet taken to Mr. Nicholson's sick room, consisting of brandy, wine, porter, cider, oysters, cheese, and crackers, his plate was supplied at the table, to an extent that startled those who observed it, with all the luxury and substantials of a profusely spread board; nor did this satisfy the cravings of his appetite; for, as Mr. Beatty informed me, he was in the habit of ordering one dish to be set aside for his benefit between meals; to wit, the back bone of the hog, cold chine being his favorite meat. Even his brother-in law was heard to express his disapprobation of his excesses.

It is by no means gratifying to me to make this exposition, nor would I in any other circumstances than those of vindicating myself under the charge of falsehood. Mr. N. might have been satisfied that I could not made this complaint except on proper advisement. It is not my custom to be kept in ignorance of the drinks and diet of my patient, nor to make my enquiries of any but those most competent to give me correct information.

From the family where he was confined I received my information, and that family recollect my remarks upon his indiscretions, when I ob-

served that Mr. Nicholson was a patient admirably calculated to disgrace his attending surgeon—language that could not have fallen from me, had it not been elicited by communications respecting his repeated improprieties. Why in the concluding part of his address he should again allude to this subject in the following condemnatory strain is truly matter of astonishment. "I defy him and the miserable beings whom he has employed, or those who gratuitously lent their aid, to propagate the charge of eating and drinking to excess against me, to substantiate it by the evidence of a solitary individual entitled to credit in the estimation of honourable men." I had been willing to concede to Mr. Nicholson the advantages of being raised a gentleman; but how shocking the contrast. After meeting with his accident he was received into the house of a friend, and there assiduously and tenderly nursed; and because, on account of the warm solicitude felt for his comfort and his speedy recovery, daily communications were made me respecting his habits, the better to enable me to exert a proper control upon his conduct as my patient, he has by one sweeping denunciation included those who were pleased to extend to him the most generous hospitality. Surely there could be no inducement on the part of the family to misstate facts; nor has there been any inclination on my part to use them improperly. But as it is possible that Mr. Nicholson may ask for names notwithstanding I have given him the source of my information; for his satisfaction I will refer him to Dr. Stephens R. Beatty of Washington county Maryland, and to Mr. Charlton Beatty of Kentucky, the latter of whom resided in the family at the period of N's confinement, and the former received his information from the family twelve months ago. The result of an application to these gentlemen, provided Mr. N. has any sense of justice left him, would be to urge him to some atonement for the outrage he has committed on truth and gratitude.

I shall not stop to argue the point with Mr. Nicholson about his falling on a piece of timber, which as he says was neither "tangible to the touch nor visible to the eye". The only information I had on the subject was second hand.

It is strange logic urged by Mr. Nicholson that because he was of mature age, therefore no secrecy on the subject of his diet could be suspected. He ought to know, however, that there are old as well as young children; and that notwithstanding his maturity in years, he acted to the life, the part of a spoiled child.

In the fourth page of his pamphlet Mr N. observes that I told him on the third week after he received his injury, that he would soon be well; when at the same time, as he goes on to state, "his leg was scaly as a fish's back," and, *as he was told*, smelt offensively; while in the seventh page he states, that on the day after going to the country his leg discharged nearly a quart of offensive matter. Now the writer might know that "scales" like those on the back of a fish do not occur during inflammation: that they are the consequence of a partially nourished integument, under the influence of pressure by the bandage; while on the contrary, the formation of nearly a quart of matter would be attended by a tumid state of the limb and a smooth distended skin;

a condition of things incompatible with the treatment pursued, as the facts in the case show.

I do not know how to answer the hotchpotch served up on the fifth page* of the pamphlet, but were I to claim the privilege there conceded to me, of having "dropt the broken bones", by which I suppose Mr. Nicholson means that I ceased to contend that the fibula had been fractured; it would place him in the ridiculous attitude of one engaged to prove by a variety of certificates, what I had gratuitously admitted; while the dilemma of a false accusation necessarily awaits him. But altogether I think this pamphlet proves, to the credit of Mr. Nicholson, that he has not been schooled to misrepresentation, otherwise he would have learned to protect a false statement by a thicker mantle.

I am willing to concede quite as much on the subject of the consultation as will answer the purposes of making out the tale of Mr. N. Not recollecting particulars, I can only add, that I had not time at that season of the year, it being during the medical session, to bestow any attentions not absolutely required of me.

It must, however, appear quite ludicrous among professional men that Mr. Nicholson should represent me as contending that the bandage had superceded the use of splints with the first surgeons "in Europe." It is a practice which I have pursued and inculcated in opposition to that of Europe for more than ten years.

In the seventh page,* Mr. Nicholson observes that matter was discharged from his leg "the day after" he retired to the country, and "in a short period after this, every muscle in my leg began to decay, and finally rotted off; my heel and part of the toes sloughed off." And again, "It was the opinion of the respectable physicians who attended me, as well as that of other intelligent persons, who witnessed my sufferings, that the whole was ascribable to the improper application of the bandages, and *that during the first forty-eight hours.*" Compare the above with the following from the eighth page. "Notwithstanding all Dr. Dudley has said to the contrary, it is an undeniable fact, which can be substantiated by several distinguished gentlemen of the faculty, that my leg was in a state of mortification when my friends removed me from under his care, extending from 4 or 5 inches above the knee to the end of my toes." The contrast of these extracts, extorts from me the concession that Mr. Nicholson must have been in a state of derangement during his confinement in Lexington, and that he has written under the influence of mental alienation. Let us put all together and see how it reads. Matter was discharged the day after leaving town; in a short time (he does not say how long) every muscle began to decay and finally rotted off, notwithstanding mortification from above the knee to the toes took place before leaving town, and consequently long before the muscles began to decay. The application of the bandage during the first two days after the injury produced consequences that did not show

*The references apply to the pamphlet as originally printed, and not to the reprint, which varies in the paging from the original.—Eds.

themselves until twenty days or more elapsed; and yet every body knows that the circulation of a part being cut off will produce mortification in eight and forty hours, as in the application of ligatures upon tumours, and in fractures where the circulation is destroyed. Lastly that the leg was mortified before leaving town, as can be proved by those who only saw it sometime after going to the country.

In regard to various other small items contained in Mr. Nicholson's publication, I decline to make any reply, and have now only to notice the certificates appended to the pamphlet.

The pert tyro may vociferate, the ignoramus may dogmatise, but it is expected of the man of true merit, that he will examine cautiously, deliberately, thoroughly; and that he will not hazard an opinion while any source of information upon the subject before him, remains unexplored. I do not propose to prefer any claims to the liberality of my professional brethren, and I waive, so far as regards myself, on this occasion, all considerations of etiquette usually observed in their intercourse with each other. To no one of the gentlemen who have signed the certificates, am I personally known; nor have I ever had the slightest connexion with either, in any shape, except with those of them who belong to the medical faculty of the University of Maryland; and who (without my solicitation) concurred in *unanimously* recommending me to the Trustees, by whom I was *unanimously* elected to the chair of surgery in that Institution. This transaction took place eighteen months ago, and consequently more than two years after the case of Mr. Nicholson was familiarly known in Baltimore.

If the conduct of these gentlemen be taken as a precedent, then I would ask of them, Who in society is safe? Certificates to prove matters of science, wherein the cause of knowledge and the advantage of humanity is the object, are admissible where truth is in jeopardy. But in the case before us, no such apology can be offered. An individual, not belonging to the profession, applies to a number of physicians for signatures to a certificate wearing mischief on its face; and I hazard the assertion, that no gentleman, whose name is on the pamphlet, can pronounce himself superior to attack with a similar weapon. However submissive his patient, under all the injunctions imposed upon him, the physician is yet liable, from numerous causes, to mortification and defeat: and is he, after the exercise of skill and assiduity, to be subjected to the painful task of vindicating himself against the certificates of his brethren, which charge him with misconception and error, and this too, not for the cause of science, but for the gratification of the worst feelings of our nature? Establish this principle, and what becomes of knowledge, and science, and manly worth? By one fell sweep, each and all will be hurled from their elevated stations in the estimation of men, while they will be replaced by every thing that is abject, ignoble, and disgusting. Once recognise the principle, and the gentlemen themselves would soon give place to ignorance and presumption, to impudence and folly. Gallileo was made to renounce his opinions in Philosophy by an impious decree; Harvey struggled for a long time against the ignorance and prejudice of the age in which he lived before he tri

umphed in the great discovery of the circulation of the blood; and in this enlightened age, when a Western surgeon proposes what he believes to be an improved mode of treating various surgical diseases, sixteen practitioners of Baltimore enter their caveat in certificates calculated to suppress all effort at improvement, and he is held up to society as unworthy of confidence or patronage. As an American some sympathy might have been anticipated in behalf of his ardor and perseverance; as a colloborator in the cause of that science, the object of which is the advancement of human happiness, the subject alone should furnish him with protection and defence, from all those who love the science of medicine as a department of knowledge, who pursue the practice with a view to fame and independence, or who are actuated by the warm and more elevated principles of the philanthropist. The natural tendency of certificates of the character in question is to repress investigation, to chill the expansive powers of the intellect, and to encourage servile and irrational imitation. Who will be found so hardy as to depart from the beaten track, when he feels that he is ever liable to be *certified* out of his reputation by his professional brethren? Will he not choose to let his patient suffer rather than suffer himself? None but the reckless would encounter the consequences of such conduct.

But let us turn to the merits of the particular case in question. In the fourth number of the Transylvania Journal of Medicine &c. Mr. Nicholson's injury is alluded to as one in which upon examination, the head of the fibula gave to my hand the sensation of egg-shells crushed between the fingers. According to Mr. Nicholson's account, the limb was amputated, seventeen months after the injury was received, by his physician Dr. Buckler, who gave a certificate a few days after that time, stating that he could perceive no evidence of fracture. Nearly three years after the amputation, viz. 9th January 1829, the leg was taken from the grave and inspected by fifteen other physicians, all of whom concur in sentiment that there was no fracture.

The question now presents itself, have those gentlemen examined the bone cautiously, deliberately, and thoroughly? I answer the question, and affirm without putting any thing at hazard—they have not.

The principle is admitted by all physiologists, that the bones as well as all the soft parts of the body are constantly under the influence of the two active systems, the secreting and the absorbing orders of vessels. Interesting exemplifications of these facts are presented in observations upon the bone which has been fractured, as well as in other instances, where no violence has been done. In the ordinary mode of dressing fractured bones, while there is an enlargement after recovery at the site of the fracture, there is also a material difference in its internal organization. At the point of union of a recent fracture, there is more vascularity than elsewhere. As nature progresses in her work of renovation by secretion and absorption, solidity takes the place of a spongy texture, and increased magnitude gives way to symmetrical form. Bones that have been irregularly set, and which present rough points near the surface after union, are rounded off by the action of the absorbents in a few months, so as to present one uniform smooth surface. Hence a dif-

ficulty, and in many instances an impossibility of pointing out the seat of a former fracture, by a bare examination of the superficies of a bone. When fractures are managed by the skilful application of the bandage, the tendency of which goes to suppress all tumefaction and inflammation in the soft parts, the increased difficulty of ascertaining, by a subsequent examination, the seat of fracture, must be palpable to the most superficial thinker; nor is it possible to decide this question with certainty, by any examination of the superficies of the bone, where the fragments have been placed fairly in apposition. The facility of retaining the head of the fibula in its place by the bandage, could scarcely be doubted by the most skeptical. There is no one point of the osseous system more accessible to the controlling influence of the bandage than the head of the fibula: hence the facility of effecting reunion with the preservation of the natural aspect of the bone.

But without any design to derogate from the anatomical acumen of the gentlemen who have placed their names to the certificates, it is proper for me to make a remark on the difficulty of deciding upon the natural form and appearances of the head of the fibula. Without regard to the size of the individual, it is large or small, flat or protuberant, protected by the tibia or standing in relief by its side; while it is completely shielded by tendinous matter inserted into it, and dense fasciæ and cellular substance surrounding it. Hence a fracture, even with a change of its natural form, would most easily escape detection after reunion; while it would be impossible to decide upon it in cases where, by the influence of the bandage or otherwise, no deformity supervened. To recognize an insulated head taken from this bone, would puzzle most physicians.

From the language of the certificates, the reader is at liberty to infer that they were signed by the gentlemen after a bare inspection of the external appearances of the bone. I have already stated that in cases where the fragments of a fractured bone are replaced in perfect apposition, and retained there by means of the bandage, no superficial appearances remain permanently, whereby to detect the site of the original fracture. But I am warranted by facts to go much farther. Neither the superficies, nor the interior organization of a bone, will betray any marks of fracture, provided the fragments are perfectly adjusted to each other, and secured there by the aid of a bandage. Such a bone examined one or two years after the injury is sustained, will be so perfectly natural in all its exterior manifestations, and so uniform in its interior organization, as to elude the most scrutinizing investigation: nor will a bone, less judiciously set, present at all times the external marks indicating the site of the fracture. On the contrary, even where considerable deformity is manifest in a bone recently fractured, every observing surgeon has seen such deformity gradually disappear, while the superficies of the bone when subsequently examined, will often be found void of all appearance of previous injury. Many causes, of constitutional as well as of local origin, conduce to change the external aspect of a bone much more than fractures, while any abrupt change in the general direction of the bone is a manifestation equally equivocal in character with others mentioned.

In order to detect the seat of a previous fracture, we are instructed by those of exalted standing, and of unquestioned skill and attainments, to make a section of the bone in question. Where the interior conformation presents a regular continuity of fibrous structure, it is impossible to make any discovery. On the contrary, if the regular fibrous texture be interrupted throughout the diameter of the bone, this is admitted as proof of fracture. In cases wherein the appearances upon making a section of the bone are not conclusive in character, a facility of arriving at truth is secured, by extracting all the animal jelly, thus leaving the part to be examined in an insulated state. But while this process enables the inquirer to detect those cases of fracture only, which occur about the middle of the bone, and wherein they are not properly placed and retained in apposition during the process of reunion, it proves radically defective in all others.

All the articulating extremities constitute so many exceptions to the mode of ascertaining the site of a fracture by the interior manifestations; and probably no one bone is better calculated to exemplify the principle, or to prove the position, than the head of the fibula. This, as well as other extremities of articulating bones, is constituted of a mass of cellular net-work bearing no analogy to the rectilineal fibrous conformation of the middle of the same, and other bones. The internal organization undergoes no permanent change as a consequence of fracture, while the superficies as well as the deeper seated parts, by reason of a larger amount of circulation, are susceptible of a more speedy return to their natural appearances. We decline to make any observations on the changes produced upon a soft bone from being buried several years in a grave yard.

Thus it is obvious, that were it made to appear that the bone had been divided, and then deprived of all animal jelly, preparatory to a more cautious and satisfactory examination, those gentlemen would not have been prepared to offer an opinion worthy of influencing the opinions of intelligent men. A blow upon the head of the bone might enlarge it, the preternatural contractions of the muscle inserted into it, might deform it; but these would be very equivocal manifestations when the bone came to be examined, in reference to the existence of a previous fracture. It therefore palpably appears, that no evidence of a conclusive character can be set forth, seventeen months after the injury, however cautious, deliberate, and thorough the examination might have been.

I am well aware that certificates are often given without due consideration, and that they are frequently rendered subservient to purposes not anticipated; and therefore have been disclaimed by their authors. Inasmuch as the physicians who have given countenance to the abuse of Mr. Nicholson are unknown to me, and have never had any just cause to lend the weight of their names to a publication of slander against me, I am left in suspense as to the course which they may esteem due to themselves, and to the profession, of which some of them are distinguished, and all responsible members.

B. W. DUDLEY.

Lexington, Kentucky, Feb. 7, 1829.

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ORIGINAL COMMUNICATIONS.

ARTICLE. I.—*A Dissertation on Congestive Bilious Fever.* By STEPHENS R. BEATTY M. D. of Clear Spring, Washington County, Maryland.

THAT beautiful writer and energetic European physician, Dr. John Armstrong has divided Typhus Fever into three varieties, viz Simple, Inflammatory, and Congestive. This division applies to the Bilious Fever of the United States, and it is from not paying attention to the different forms, that we find such a discrepance of opinion among Medical Writers on this subject, as to the proper treatment; that which is appropriate to one form being entirely unsuitable for the others, and that which is successful in an epidemic of one year, inefficient in an epidemic of the following; as it may prevail in a simple form one year, the inflammatory the next, and the congestive the third; or all three may exist during the same epidemic, and in the same neighbourhood, one patient having the simple, another the inflammatory, and a third the congestive; according as each has been exposed to a less or more malignant form of the predisposing cause, m rsh miasmata, or exciting cause, as cold, fa-

tigue &c. or depending on some peculiarity of constitution of the individual.

To draw the line of distinction at one view between these varieties, I shall first give a short outline of the simple and inflammatory, and then proceed to the main object of my essay, the congestive.

The simple bilious fever is that in which the predisposing cause marsh miasmata, the exciting cause, whatever it may be, or both combined, are of so mild a nature as to depress the action of the heart so little in the stage of oppression, that a proportionally moderate reaction readily takes place; and the viscera of the patient being in a sound condition, it passes through the stage of simple excitement without any symptoms of topical inflammation, and if properly managed almost universally terminates in health. Blood-letting is seldom required in this form of fever, yet it may be resorted to with safety and advantage, whenever the pulse becomes too hard and strong; particularly if attended with much uneasiness in the head, as inflammation may be thereby prevented from occurring. It soon yields to repeated doses of calomel assisted by other purgatives, cold ablutions &c.

The inflammatory bilious fever is produced by the same causes as the simple, but of a more powerful degree. The stage of excitement consequently runs much higher, and is accompanied with inflammation in some one or more of the viscera, sometimes arising with the fever, but often caused by the violent action of the heart and arteries on the predisposed organ or organs, during the stage of excitement. It is this variety of fever which demands an energetic and effectual use of the lancet, calomel assisted by other purgatives, cold affusions, blisters, specific effect of mercury &c.

The congestive bilious fever, from the coldness of the surface which attends it and from its fatality, has received the vulgar appellation of cold plague; and in the winter and spring being accompanied with pneumonic symptoms is called typhoid pneumonia. Having met with a considerable number of cases of this form of fever, some of them of a very violent character,

during a practice of four or five years on the Potomac river in Maryland, I shall in the following pages give a short history of the disease and point out what I consider its proper treatment. While the remote causes of the two former varieties are comparatively mild, the mal aria, and the exciting causes producing the congestive form, are of a grade so overwhelming and deadly as to prostrate at once the action of the heart and other constitutional powers, to such a degree that reaction either does not take place at all, or if it does it is only partial or transient. If not early arrested by proper treatment, it soon runs on to the stage of collapse without an intervening one of excitement, as in the two former.

SYMPTOMS.

The principal external distinctions between the two first varieties and the congestive, are that in the simple and inflammatory the skin over the whole surface is hot and dry, the pulse more or less full, strong and hard; whereas in the congestive the surface is as cold as marble, particularly on the extremities, and often bedewed with a clammy sweat, the patient at the same time complaining of great heat internally, in some cases so intolerable as to cause him to cry out that he feels as if a furnace was burning within him, and constantly to be calling for cold water to relieve the intense thirst and internal heat; while at the same time the pulse is frequent, weak, struggling, and unresisting. The attack often comes on suddenly; at other times more gradually, the patient first complaining of lassitude as in other fevers; the extremities then become cold; the stomach so irritable as to reject every thing it receives; if a glass of cold water, so grateful to the patient, be taken, it is immediately thrown up; if medicine, the same thing occurs. There is great anxiety at the præcordia, pain in the head, and anxious and difficult respiration. The tongue is often thickly coated and the bowels torpid, all the secretions being either totally arrested, diminished in quantity, or of a vitiated quality. The tunica conjunctiva and skin often become of a bright yellow colour: some cases occurred to me in country situations on the Potomac in the fall of 1823, attended with such violent symptoms and yellowness

of the skin, as would have ranked them amongst the worst cases of yellow fever in those cities where this disease mostly prevails. In all the cases of congestion which I have met with, a symptom occurred which I have never seen noticed by any author—a numbness and deadly feeling commencing in the extremity of the fingers and toes, and proceeding gradually up the arms and legs, evidently caused by the recession of the blood from the extremities towards the internal part, as the congestion increased. The patients pointed out this symptom, and were never satisfied unless some person was constantly rubbing each limb, as they themselves said, "to keep the blood circulating."

Sometimes partial reaction comes on, some part of the body or the head becoming preternaturally hot. If it be the latter, the carotid arteries pulsate strongly. A general excitement may emerge, particularly after the action of proper remedies, and the disease then runs its course under the simple or inflammatory form and must be treated as such.

PATHOLOGY.

The first effect of the remote cause of the simple and inflammatory fevers is to diminish the heart's action. It being therefore unable to propel the blood to the extremities with sufficient force, a consequent internal accumulation of that fluid takes place, and the surface becomes cold, constituting the chill. This internal accumulation of blood, the natural stimulus of the heart, at length excites it into action, and an open fever is produced, the blood being thrown to every part of the body and a general heat excited on the surface. But in the congestive fever, either the predisposing cause, marsh miasmata, the exciting cause, or both combined (as before stated), are so deadly and overwhelming, as almost completely to prostrate the action of the heart; a more extensive congestion takes place about its right side than in the other two varieties, and its muscular parietes are so much distended by this unnatural engorgement as to be almost paralized; for the same reason that the contractile muscles of the hand or mouth or any other part, when they are stretched to their utmost extent, have not near so powerful a

contracting force as when they are only moderately extended.

The heart therefore in this fever has not the power of producing a general excitement, as in the two former: but efforts towards it are sometimes made, the temperature of the chest or abdomen being raised considerably above the the natural standard; while the other part of the body remains of a death-like coldness, bedewed with a clammy sweat, and the pulse at the wrist scarcely perceptible, the powers of the heart being still too weak to extend its influence so far from the centre of circulation. This attempt at reaction generally vanishes in a short time, the heart being too much oppressed by the internal engorgement to accomplish it, and the disease passes on to the stage of collapse and terminates in violent cases as soon as the third day: in others it lasts nine or ten, or even longer. The congestion commencing in the right side of the heart, extends up the descending cava into the sinuses and veins of the brain, producing the sense of fulness of that organ, coma, delirium &c. In the thorax the blood returning from the vena azygos, and through it from the bronchial veins being obstructed by the accumulation in the descending cava, and the congestion of the right ventricle extending into the pulmonary arteries, oppress the lungs and thereby cause that anxious respiration and sense of suffocation which invariably attend this disease.

To have a correct idea of the extensive congestions in the viscera of the abdomen; it is necessary to take a view of their venous circulation. The veins of the stomach, pancreas, spleen, and intestines are all collected into three trunks corresponding to the three great branches of the abdominal aorta, the cæliac, superior and inferior mesenteric arteries from the ramifications of which they are supplied. These are again collected into one called the vena portæ, which enters the liver between the processus caudatus and lobulus spigelii. After entering the liver the vena portæ, resembling in this respect an artery, divides into two branches, one going to supply the right, and the other the left lobe of the liver, and terminates in the extremities of the vena cava hepatica and pori biliarii. This vein therefore is not unaptly compared to a tree, the ramifications coming from

the stomach, pancreas, spleen, and intestines, representing the roots, the vena portæ the trunk, and the minute ramifications in the liver the branches. The hepatic veins passing through the liver towards the heart, are collected into two great trunks which enter the ascending cava as it pierces the diaphragm to enter the right auricle. That portion of the blood of the vena portæ, which remains after the secretion of bile from it in a healthy state, passes through the hepatic veins to the heart. In the abdomen then the congestion extends from the vena cava ascendens, through the vena cava hepatica, into the substance of the liver, producing an engorgement of that important viscus. Excessive venous congestion in an organ, suspends its secretory function as completely as an unnaturally increased arterial action of that part: the current of blood of the vena portæ through the vena cava hepatica being obstructed, and that part of its blood which in health is used for the secretion of bile being now not called for, it must necessarily be congested, and the congestion must extend to all the viscera of the abdomen, which furnish the portal circulation, viz. the stomach, pancreas, spleen, and intestines. The kidneys discharge their blood into the ascending vena cava and consequently suffer from the internal accumulation. From this view of the subject, it is an easy matter to account for the derangement which takes place in the abdomen; as the entire suspension, diminution, or vitiated state of the biliary secretion, the irritability of the stomach, the torpid state of the bowels, and paucity of urine.

It is now plain that in the congestive state, too large a proportion of blood is accumulated in the veins and right side of the heart, and of consequence too small a quantity remains in the left side and arteries. This state explains the cause of the extreme feebleness of pulse and general coldness of the surface, the characteristic symptoms of congestion. Haller has estimated that in health the veins circulate three fourths of the blood, and the arteries the remaining fourth. If this be correct the veins may in a case of violent congestion contain four fifths, leaving a remainder of only one fifth for the arteries, a complete loss of balance in the circulation.

TREATMENT.

Bloodletting. Having assumed Dr. Armstrong's division of fevers, I must differ from him in opinion as to the propriety of bloodletting in one of the forms. It is always extremely unpleasant to differ from such a man on any point, and particularly on a practical one; but in doing so I have the authority of every medical man with whom I have conversed on the subject, added to my own experience. However I may laud his recommendation of an energetick use of the lancet in the inflammatory variety of fever, yet I am convinced that in advising its use in the congestive form he is wrong. I venerate the writings of Dr. Armstrong as much as those of any modern writer, but in this point I must differ from him. His arguments in favour of its use are very ingenious and upon first view plausible; they were convincing and satisfactory to my mind, until I found in practice it did not answer the purpose for which he advised it.

He starts upon the ground that the debility in congestive fever is apparent, not real, the strength being only suppressed by the internal accumulation of blood;—so far he is correct: the first remedy which he advises is the use of the lancet *to unload the congested veins* and thereby remove the oppressing cause. This at first view would appear to be the most efficient plan for accomplishing this desirable object, but I shall attempt to show upon anatomical facts that instead of diminishing it increases the congestion. He says "the balance between the arterial and venous systems is more or less disturbed in every instance of congestive fever; for there is more blood accumulated in the veins, and of course less contained in the arteries than in a natural state."* Now if blood-letting had a tendency to relieve this state, it would be a proper remedy: but what are the facts? A bandage is thrown around the arm and a vein opened; every one at all acquainted with anatomy knows that there are valves every few inches in the veins of the extremities, preventing the flow of blood from within, where the fulness exists, outward; all the blood then which flows from the orifice is supplied by

*Armstrong on Typhus p. 75. Philadelphia 1822.

the veins between it and the hand: and what supplies the hand? the branches of the humeral artery. The internal engorged veins are thus left in the same state in which they were, and blood is extracted from the arteries which the Doctor himself has admitted to contain "less than in a natural state": the inevitable consequence of this must be to reduce still more the action of the heart, (already so weak as scarcely to be able to force the blood to the surface, as is proved by the feeble pulse and cold surface) and by this reduction of its action increase the congestion.

Again, he says "often the blood merely trickles, or rather oozes from the punctured vessel for a considerable time, being much darker and thicker than natural. Yet when a few ounces have been drawn it usually flows with freedom and becomes finally of a brighter colour. In congestion the pulse generally rises under or immediately after blood-letting, *but sometimes it continues oppressed, and even weaker than before and the bleeding ought not to be further employed*; for as the rising of the pulse under blood-letting in congestive fever is a certain indication of its propriety, *so its sinking is as certain an indication of its impropriety*, and therfore we have a *criterion to guide us in the operation.*"* I have seen blood-letting practiced in congestive fever, and I have practiced it myself, and never yet have I known the pulse to rise during the flow or after it, as stated by him, nor can I conceive any reason why it should. The left side of the heart and the arteries are admitted to contain less blood (their proper stimulus) than in a natural state, and I cannot understand how reducing the quantity still lower, as I have shown blood-letting, from the arm at least, does, can increase their action; but on the contrary I have invariably found the pulse to sink during the flow, to remain more feeble and oppressed than before the operation, the surface become colder, and the respiration more anxious and difficult. No rule is given by which we are to judge of the propriety of the operation, until after it has been performed. He says "the pulse sometimes continues op-

*Ibid p. 143 and 144.

pressed and even weaker than before, and the bleeding ought not to be further employed:" but the mischief is now done, and it is too late to determine on its propriety or impropriety. When I first read Dr. Armstrong's work, I was convinced of the propriety of blood-letting in congestive fever, and pursued the practice in the first cases which I met with, until I found it followed by the effects above stated. I then began to examine more particularly into its modus operandi, came to the conclusion which I have stated, and abandoned it altogether. I have since treated the disease without bleeding and have been pleased with the result, all the cases terminating successfully; but candour compels me to state that the latter cases were not so violent as the first, which may in part account for the more favourable issue.

After all that has been said in its favour by Dr. Armstrong, he appears to have doubted of its propriety in some states of congestion. In his recapitulation (p. 385) he says "If we take the very extreme cases of venous congestion, those in which the causa morbi is at once so overwhelming as really to sink the constitutional powers for a time, we should be disposed to defer evacuations until the first shock was over; as we find is done every day by the best practical surgeons when the force of the heart is subdued, and the animal heat reduced every where below the natural standard, by the great shock of a fall from a height or other accident. In such cases a cautious use of stimulants is indicated, to rouse the heart from its torpid state; but as soon as reaction takes place, and the pulse and the temperature of the surface become elevated above the natural state, blood-letting is necessary to control them, and prevent inflammation from occurring. I cannot see why the same principles should not apply to congestive fever, as the state of the system is the same in both cases." The best general rule for the use of the lancet is the state of the pulse: if this be strong, full and tense, it is called for; if on the contrary, it be feeble and unresisting, it is not: there are some few exceptions to this general rule, as in the depressed pulse of gastritis and enteritis.

After the use of the remedies to be presently named, reac-

tion comes on, and an open fever emerges. If the excitement should run too high, blood-letting must then be resorted to, to control it. It is now to be considered either a fever of simple excitement, or an inflammatory fever, according to the symptoms, and must be treated as such: but considerable caution is even yet necessary; for if it is resorted to before the excitement is fully developed, the system is again thrown back into a state of congestion, as I have more than once seen.

We have the testimony of Dr. Cartwright to the same effect. In his essay on the yellow fever of Natchez in 1823, he states, "If blood-letting were resorted to before the general reaction, the reaction which had only yet partially developed itself, abated; the heat which had begun to spread itself over the surface disappeared; and the pain (which always accompanied the reaction) would subside; and this too, before the quantity of blood taken away was in any degree considerable. Medicines did not operate and the disease at once passed into the last stage. During the ataxic reaction, in vain may we be told to bleed for the relief of the symptoms; for *they grow worse while the blood is flowing* and continue to become more alarming, and that in proportion to the quantity of blood taken away."

Having given my objections to blood-letting from the arm in cases of congestion, I shall now make some remarks on the performance of the operation in another part of the body. Mr. Charles Bell in his anatomy says, "the external jugular vein, where it is before the mastoid muscle, forms free communications with the internal jugular." If this be an anatomical fact, an orifice in the external jugular vein would directly unload the right side of the heart, through the internal jugular and descending vena cava, (no valves existing in them) of that superfluous blood, which by its distension prevents the proper contraction of that organ. Or, if no valves exist in the external jugular, which I believe is the case, another channel is offered through the subclavian vein, into which the external jugular empties, by which the same effect would take place in case of great fulness extending up the external jugular. I have never tried thisplan; but should think it well calculated to relieve immediately that part

of the circulation, where the unnatural accumulation exists, without reducing the quantity in the left side of the heart and arteries where too little exists; but on the contrary, increasing it indirectly by removing the obstruction to the circulation in the right side. As far as one case can go to prove the correctness of what has been stated, the following is in point. Dr. John E. Cooke, Professor of the Theory and Practice of Medicine in Transylvania University, relates in his lectures the case of one of his patients to whom he was called in haste, and found her in a comatose state, and supposed by all present to be in articulo mortis. The pulse was feeble and oppressed and the surface cold: discovering an enormous distension of the external jugular veins, he immediately opened one: the pulse rose during the flow of the blood, the patient opened her eyes, and was completely restored from the coma.

TARTAR EMETIC AND WARM BATH.

Although I have never employed the first of these remedies myself in this disease, yet from the strong terms in which it is recommended in that peculiar state of the system of which I am treating, by Dr. Dudley, Professor of Anatomy and Surgery in Transylvania University, who has given it in this disease for many years, I should advise the use of it as the first remedy to be employed in congestive fever, in combination with the warm bath. This medicine has also been extensively and successfully used for a few years past in the congestive form of yellow fever at Natchez, by Dr. Cartwright who was a pupil of the Transylvania School. In every case of congestive fever, particularly if a violent one, the practitioner should order a warm bath to be prepared as quick as possible, and the patient to be put into it as soon as it is ready. In country practice where nothing more convenient can be obtained, a large tub or barrel will suit very well for the bath: its temperature is to be regulated entirely by the feelings of the patient, as a bath which feels disagreeably hot to a person's skin in health, will impart the sensation of coldness to that of a person suffering under a violent attack of this disease: the addition of a small quantity of salt to the bath will increase its activity on the skin, and by its irritating quality greatly assist in

bringing the circulation to the surface. Doctor Dudley's method of giving tartar emetic in this disease, is in small and frequently repeated doses, dissolved in a very minute quantity of water, so that the medicine may exert its full influence on the stomach before vomiting be excited. Ten grains should be dissolved in twenty tea-spoonfuls of warm water, and one tea-spoonful given to the patient about every five minutes until free and copious vomiting takes place. By giving the medicine in this manner, time is allowed for it to have its full influence on the stomach, before a sufficient quantity is taken to produce emesis. If a full dose is given at once, ineffectual vomiting is immediately excited before the medicine has had time enough to have its proper effect; and the irritability of the stomach, instead of being relieved by it, is increased, for the same reason that the too hasty operation of a purgative increases instead of diminishing the irritability of the bowels in dysentery. Should the medicine given in the above manner increase the irritability of the stomach and the ineffectual efforts at vomiting, which generally attend this disease, a few drops of laudanum should be added to each tea-spoonful, to cause it to be retained until a sufficient quantity be taken to produce copious emesis. The effects of tartar emetic in this disease, when it produces free and copious vomiting, are, to rouse the heart from its torpid state; to drive out the blood to the surface, restoring to it its natural heat; to excite the secretions which were all suspended or greatly diminished before its use; and finally to relieve entirely the excessive irritability of the stomach, thereby facilitating the administration, retention and operation of other remedies.

It would be desirable that vomiting should come on while the patient is in the bath, as we would then have the co-operation of two powerful remedies, one from within, the other without, in restoring the lost balance of the circulation and imparting heat to the skin. But if vomiting should come on before the bath be ready, it may still be used, provided the temperature of the surface and state of the circulation should require it. After free vomiting has commenced, copious draughts of warm water should be taken to prevent the injurious effects on the stomach,

which, according to Orfila, are apt to ensue from too concentrated a solution of the medicine.

Dr. Cartwright, in his excellent essays* on the Malignant Fever of Natchez and its neighbourhood, in the years 1823 & 1825, speaks in the highest terms of the use of tartar emetic in the congestive stage of yellow fever. His method of giving it was somewhat different from the above. He says, "I generally gave from six to ten grains at a dose, and repeated it in smaller doses until it either produced bilious emesis, removed the chilly feelings, developed an excitement throughout the system, produced secretion in the skin or kidneys, subdued the ataxic fever, or palsied the deranged organic actions." No apology is necessary for giving a few more extracts on the use of this medicine from these essays. "In the first stage of yellow fever, or in that which consists of an ataxic or crippled reaction, when the blood is unequally determined, the heat unequally diffused, sensation impaired and secretion suspended, *I found no other remedy* or combination of remedies, which produced such decided effects as tartar emetic in full doses." Again: "But it was not always that tartar emetic, when given in the first stage, would produce vomiting. In those cases in which in the first stage the skin was cold, and when the reaction was ataxic and scarcely perceptible, the organs as if palsied, secretion entirely suspended or strangely vitiated, the stomach irritable, and little or no pain complained of, tartar emetic in full doses might be given without producing vomiting. Strange as it may appear, tartar emetic in such cases was a most powerful stimulant. It brought out the excitement, heated the skin, raised the pulse, allayed the irritability of the stomach, restored sensibility to the organs, and finally awakened one or more of them, the kidneys, skin, &c. to active secretion." Lastly: "It will be remembered, that those which are denominated *cold cases* are the most hopeless. I have used the hot bath, frictions, sinapisms, blisters, besides various internal stimulants, to bring on reaction; but the combined influence of all these remedies has never had the same beneficial influence as tartar emetic alone."

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*Medical Recorder, Nos. 33 and 34.

RUBEFACIENTS AND BLISTERS.

After the patient is removed from the warm bath, if the extremities should remain cold, pulse weak, &c. the feet and hands should be wrapped up in large mustard poultices; active friction should be made on the legs and arms with red pepper dissolved in spirits; bottles of warm water or warm bricks, should be placed to the legs and arms, and the surface stimulated in every possible manner. If the stomach remain irritable, a mustard poultice must be placed on the epigastrium. In the worst cases, the insensibility of the skin is so great as not to feel the impression of blisters at first. I have known them to remain for days without raising the cuticle, but as soon as its heat and sensibility are somewhat restored by other means, blisters to the ankles and wrists will assist in stimulating the heart, and inviting the circulation to the extremities by counter-irritation.

STIMULANTS.

In violent cases in which the action of the heart is almost completely suppressed, stimulants should be freely given, as wine, rum, brandy, &c. to keep it in play until other remedies have time to act. Enemata of the oil of turpentine will also greatly assist in accomplishing the same object.

PURGATIVES.

We come now to that class of remedies upon which common reliance is placed for a radical cure of this disease. I have shown in the pathological view I have taken of it, the extensive congestion which exists in the portal circulation, and in all the viscera of the abdomen which supply it. There are only two outlets by which the vena portæ can be unloaded, one through the hepatic veins, and the other by the secretion of bile. The first is obstructed by the congestion of the right side of the heart and ascending cava, and the second by the entire suppression or diminution of the biliary secretion. If then by any means a copious secretion of bile can be effected and kept up for some time, the congestion of the whole portal circulation will be relieved, and, a less quantity of fluid being thereby left to pass through the hepatic veins, the relief will extend to them, to the ascending cava and heart, and the whole circulation will be freed in proportion to the quantity of bile secreted. This suspended state of the biliary se-

cretion accounts very plainly for the yellow colour of the adnata and skin in bilious disorders; that part of the portal blood which is intended for the formation of bile, not being used, must of course pass on through the hepatic veins and enter the general circulation, imparting a yellow tinge to the whole surface and every secretion of the body.

At the head of that list of remedies whose action effects the above object, is calomel. As as soon the vomiting has ceased and the stomach become tranquil, a scruple dose of that medicine should be administered, and its action on the bowels assisted after a few hours, by some other purgative, as jalap, if the stomach will admit it, or scammony, aloes, castor oil, &c. In cases in which the irritability of the stomach remains after the use of tartar emetic, calomel alone should be administered in the form of pills; as by their weight they will be retained better than if it be given in the form of powder involved in syrup. Much has been said of late years on the necessity of large and repeated doses of calomel and other purgatives to produce an action on the bowels in congestive and other severe attacks of bilious fever. It is said by those who have been in the habit of giving tartar emetic in this disease, that its previous use by restoring the natural sensibility and removing the morbid state of the stomach and bowels, and equalizing the circulation, supersedes the necessity of those enormous doses of calomel and other purgatives, which, without it, have been found necessary to move the bowels. If this be the case, it is an invaluable remedy, as the mercurial practice when carried to such an extent, is always attended with danger of profuse salivation and sloughing. If the medicines given to assist the calomel, should be rejected, prove ineffectual, or produce watery discharges, which they sometimes do, our sole reliance must be placed on calomel. I have never myself given more than a scruple dose of this medicine, but in some cases have found it necessary to repeat the dose as often as three times, the stomach not retaining any thing else, before I could accomplish the object I had in view, that of bringing away consistent black and bilious discharges; but I should not hesitate to extend the dose of it or any other suitable purgative, to any amount that may be found necessary to accomplish the object

above stated, upon which depends the safety of the patient. In giving any medicine, we have an object in view which we wish effected by it, and until that effect be produced I cannot conceive of any reason why we should cease giving it.

It is astonishing to witness the immediate relief which follows the free operation of a mercurial cathartic. I have known the difficult and anxious respiration to vanish; the pulse from being feeble and oppressed, become open, full, and soft; the skin become warm, and the numbness and deadly feeling of the fingers and toes, disappear, as soon as the operation had taken place.

The calomel and other purgatives, either separately or in combination, should be repeated daily, and continued until the congestion be entirely removed, and the biliary secretion become healthy. If the discharges from the bowels become thin and watery, calomel alone must be again resorted to, and assisted again by castor oil, if it does not increase them; as all other discharges are injurious except those that are consistent and bilious. The latter coming through the ductus choledochus relieve the congestion, while the watery discharges being a secretion from the exhalents of the intestines, which proceed from the arteries, weaken the action of the heart, in the same manner as blood letting, and thereby increase the disease. I must again repeat, that all discharges which proceed from the internal engorged veins, as bilious ones do, are beneficial by bringing away a part of that load which oppresses the heart; while all which come from the arteries and left side of the heart, are injurious by reducing the quantity of blood still lower where too little already exists.

Should general excitement emerge after the use of purgatives, or even after that of tartar emetic, and run too high, resort must be had to the lancet, and the disease treated in all other respects as one of simple excitement, or inflammation, according to the symptoms. But should the stage of collapse be approaching, even active purgatives would be dangerous, and we must then endeavour to obtain the specific effect of mercury as quickly as possible, as nothing else has so good a tendency to equalize the circulation and restore the secretions to their natural state. A slight ptyalism should always be considered a happy occurrence in a violent congestive fever.

ART. II.—*Miscellaneous Cases.* By James Conquest Cross M. D. of Courtland, Alabama.

CONTINUED FROM PAGE 111.

CASE VI.—*Erratic Inflammation.* In January 1827, I visited Mr. W. C. of Fayette County, Kentucky, æt 56; of emaciated habit of body, and sanguine temperament. I found him labouring under common colic, so violent in its character as to induce me to give him immediately ʒj. of laudanum. This was repeated every half hour until he had taken ʒiij, which entirely relieved him of pain. Ol. Ricini and purgative enemata were directed to be given until his bowels were freely opened; as they were then, and had been for several days, in a state of constipation. My advice was neglected, and I was summoned the next evening, and found his bowels locked up and him labouring under a very distinctly marked pleurisy. The pain was in the right side; he was bled ʒxvj.; a cathartic composed of fifteen grains of calomel and eight of aloes, was administered, and enemata occasionally used to accelerate their operation. The next day I found him better, the cathartic and clysters having procured several copious alvine evacuations. There was still, however, pain and a tolerably full and hard pulse. He was bled ʒxij; a large blister was applied to the pained part; oxymel scillæ as an expectorant was given; and his bowels to be regulated by the occasional use of calomel and aloes. At 7 P. M. all the symptoms had subsided, and he appeared free of disease. About mid-night, however, I was again summoned, and found him labouring under severe pain of the left side, with the usual accompanying pleuritic symptoms. This modification of the disease was treated nearly in the same manner, though not quite so energetically, and yielded in the course of three days, when I flattered myself the work was finished. But thirty-six hours after I considered him convales-

cent, I was again called, and found his pulse tense and full; his abdomen tumid and very painful when pressed; his stomach irritable; his bowels constipated; his skin feverish, and his tongue covered by a white furry streak from near its root to within about three quarters of an inch of the point, while the edges and apex were of a deep crimson. I bled him ʒx. only; apprehensive from the depletion he had previously sustained, together with his advanced age, that he could not bear the loss of more, without running the risk of producing dangerous prostration. Twenty grains of calomel were given, which immediately quieted his stomach; and while speaking of gastric irritability, let me remark, that I have found from one scruple to a drachm of that article, superior to every remedy I ever tried, in tranquillizing the stomach when rendered irritable by any cause whatever. A large blister was applied to the epigastrium and he was bled in small quantities, i. e. from 4 to 8 ounces daily for five days. His bowels were kept under the operation of aloes, calomel, rhubarb and senna during the space of a week, at the expiration of which time I again thought him convalescing. Five days had elapsed when I was again summoned, and found him labouring under irritation or inflammation of the urinary bladder, but which precisely, I was unable to determine. The hypogastric region was swelled and somewhat painful; his urine flowed stillatitiously, and although there existed symptoms of general irritation, his extremities were not cold; there was no delirium nor did he vomit. Small and repeated bleedings; calomel and opium; emollient injections, in conjunction with a few adjuvants of trivial importance, procured him perfect relief in the course of forty-eight hours. From this time he continued without interruption to improve until June, though feeble, and infirm, when he appeared to enjoy as good health as men usually do at his advanced age.

Remarks. This case I have recorded exclusively on account of its singular erratic character; though I confess my inability to offer any satisfactory suggestion, explanatory of the principle upon which it depended, or the cause to which it was indebted

for that peculiarity. It does not appear to me to be amenable to any of the laws which control the developement, march or termination of abnormal action. That the different attacks, assuming sometimes similar, and sometimes dissimilar types, were connected, no one I am persuaded, will venture to doubt; the principle, however, upon which this connexion is to be explained will not admit of the same unequivocal certainty. The idea doubtless will spontaneously arise in the mind, that the pathological states of the system just described should be arranged under the head of *metastasis.* A little reflection, however, must effectually subvert this opinion. It sometimes happens, that the texture or organ originally affected ceases suddenly to complain, and another remotely situated, or a distant part of the same tissue, manifests immediately disordered action. When the cause producing this result is known, it is denominated in the language of humoral pathology *revulsion*, but when unknown *metastasis.* This is a distinction too metaphysical, and answers no practically useful purpose; for the effects produced are identical. This distinction, however, is made by Roche and Sausen, and embraces what they have furnished us on this subject.

When we refer to the details of the case, we find it according to this definition, defective in a most essential attribute; for instead of morbid action springing into being in a distant part the moment of its cessation in the tissue originally affected, we see hours and even days elapsing before the parts, supposed to be metastatically diseased, began to complain. Why a knowledge of the cause should establish a distinction, when there is no visible or appreciable difference in the effects produced, is inexplicable: our ignorance, therefore, of the agent that in this case imparted to it its erratic character, would not lend support to the idea, that it was one of metastasis. That no phlogistic state of the vascular system existed after the first pleuritic attack, is clearly evinced by the depletion sustained by the patient; no cognizable cause of any kind obtained as far as I could discern, and indeed when we take an *a priori* view of the several attacks, the copious detraction of blood, together with the other active depleting remedies, would lead us to the unavoidable conclu-

sion, that if disease of any kind supervened, it would be rather, one of diminished than of phlogistic action. The explanation of metastasis given us by Broussais, is infinitely more simple, natural and comprehensive than that of Roche and Sausen; indeed it surpasses in these respects, every other definition that has fallen within the sphere of our researches. We daily see textures diseased without extending their influence to other parts; this arises from the very feeble sympathies either normal or abnormal of the organ affected. When, however, its sympathies are either numerous or active, other tissues become diseased, and generally in the same way, but they are not necessarily proscribed to the same degree of intensity. Thus, Broussais, remarks in his Propositions or Principles of Medicine, that "The organs sympathetically irritated may contract a higher degree of irritation than that of the organ originally affected. In such cases the disease changes its name and place." This is the aphoristic definition of metastasis of Broussais, which we esteem so worthy of high praise; and if it be technically precise, of which we entertain no doubt, the reader must readily discover from the history of the case as recorded, that it is essentially defective in one of the most conspicuous attributes of this definition. 'Tis true, that the pleuritic symptoms were more intense than those that previously obtained, and which pointed out the existence of colic, but those that disclosed disease in the left side, were not more severe than those of the right, nor were those of the abdomen, more violent than those of the chest, while those indicating disease of the urinary bladder, were moderate, when compared to those that characterised inflammation in either of the tissues just alluded to. Moreover, although it always changed its place, on one occasion, it did not change its name, a circumstance which the author just quoted, seems to consider essential and invariable. We shall perhaps be more correct when we say that the case under consideration is embraced by Broussais' *Propos.* LXXIV. *L'irritation sympathique est de meme nature que l'irritation primitive.* Thus, whenever an irritation in a distant part becomes abnormal, it puts on the livery of the disease from which it emanated:

if from an inflammation it becomes inflammatory; if from a bleeding it becomes hæmorrhagic &c. There would be no hesitation, in assigning this case to this head, were the law itself founded in truth, and of its truth a litle consideration will enable us to see sufficient reason to doubt. Thus, we sometimes discover, that an irritation transmitted from one organic system to another, does not produce identically the same disease. This is especially the case with regard to hæmorrhagic irritations. Thus, we see a bleeding, the sympathetic effect of inflammation, and vice versa. There is danger, however, of confounding these phenomena with the supplanting of one irritation by another, when there can exist no sympathetic connexion between them, as under such circumstances, the nature of the primitive disease can exercise no influence upon the nature of the consecutive. It is thus, when there indifferently succeed to a suppression of hæmorrhage an inflammation, a nervous affection, a secretory or nutritive irritation or even another hæmorrhage. This proposition or law, will be found still more defective, when we turn our attention, to those secondary irritations that arise in textures unlike in structure and function to that in which the primitive affection was seated. Thus, an inflammation of the skin and of the mucous membranes, is apt to produce sub-acute sympathetic inflammation in the corresponding lymphatic ganglions: a pustular inflammation of the dermoid organ when transmitted to a mucous membrane rarely assumes that character, &c.

Case VII. *Peritonitis.* In the spring of 1827, I was called to a negro boy æt. 14, the property of Major Matthews Flournoy, and found him complaining of intense abdominal pain; belly swelled; pain much increased on pressure; bowels costive; tongue furred in the middle and red at the edges; pulse small and feeble; recumbent upon his back, with legs flexed upon his abdomen which he was unable from the excessive pain to exend. Bled him to ℥xvi. blistered his abdomen and gave him of calomel grs. xx. and of opium grs. ii, with directions to repeat the dose if the pain did not subside in two hours. I remained with

him about an hour when he was able to extend his legs, so great and prompt had been his relief. The second dose was given at the time specified, which procured him complete relief from all pain almost immediately. The ensuing morning oleum ricini was given which completed the cure.

Remarks. This case is recorded to illustrate the great efficacy of calomel in large doses in removing inflammation of the serous membranes. We are aware, that this article in conjunction with opium has been recommended in phlogosed states of the serous membranes by others, and particularly by Dr. Armstrong. The latter, however, who justly enjoys the credit of introducing this important remedy, gives, as I conceive, the mineral in too small a dose; not too small 'tis true to answer the purpose ultimately, but too small to accomplish the cure, as certainly and as quickly, as when given in larger quantity. Mr. Annesley has by his experiments put beyond the reach of cavil, the virtues of calomel in subduing inflammation of the mucous tissues; and my clinical observation could furnish me with several strong cases illustrating its power over inflammation of the serous membranes. The lancet and epispastic no doubt had their share in this case; others, however, might be adduced, in which neither of those remedies interfered, and which yielded promptly to the exclusive agency of calomel and opium. To an adult I never give less than half a drachm, and have given as much as a drachm at a dose, to be repeated if necessary at the expiration of two hours. I rarely find it necessary to give more than two doses. I selected this case simply because it was the first in which I ventured to give a scruple dose, the others may appear hereafter in a more systematic form.

Case VIII. *Peritonitis.* Last April I visited Mr. C. a mechanic of this place, and found him labouring under peritonitis. He is æt. 45; sallow complexion; sanguineo-bilious temperament, and thin habit of body. On the 10th he had been exposed to a heavy shower of rain, and after spending a very restless night was attacked on the morning of the 11th with excrucia-

ting abdominal pain, which he mistook for colic and which induced him to postpone employing medical aid till 10 A. M. at which time I saw him. So exquisitely torturing was his pain, that he could not bear the most gentle touch of the finger without screaming; his belly was tense and enormously swelled; pulse full, hard and rapid; bowels costive. Bled him ʒxxx, and gave him two scruples of calomel, and three grains of opium. At half past 10 A. M. pain and general suffering rapidly increasing. Bled him ʒxvi. At 11 no alteration for the better, but if possible getting worse; stomach now irritable; great physical exhaustion; perspiring literally in streams from every pore and cold and clammy in its character. I waited till 12 o'clock without observing the least pause in the progressively increasing violence of the disease, and then gave him five grains of solid opium. Several minutes elapsed without gaining any ascendency over the disease, and apprehensive from its intensely violent character that death would soon take place, I had Doctor Booth called in consultation. When he arrived, the narcotic had been taken twenty-five minutes; he suggested the application of a blister, but before it could be administered, the patient observed that he was much better, and that he could scarcely feel any pain. I uncovered his abdomen and was surprised, that I could handle it with considerable rudeness, when but a few minutes before, the slightest touch extorted from him cries that attracted the attention of the whole neighbourhood. The blister was applied, and at 1 P. M. he was calm, collected and free of disease; he complained of no pain and the abdomen had lost its drum-head tension. During the remainder of the day, he slept well and was easily roused, the opium not having had any deleterious influence upon his system. His bowels were still costive, not having been operated upon during the whole day, and it required repeated doses of castor-oil and frequent enemata to induce them to yield. In the morning they were sufficiently soluble, and the patient free of every species of uncomfortable feeling.

Remarks. This case exhibited uncommon violence, and a

violence too of such a character, as to induce me at one time to believe, that dissolution would supervene before the system could be brought under the controul of any remedy. It yielded, however, with a promptitude truly astonishing, and only commensurate with the frightful intensity displayed in its every feature. The detraction of ʒxlvi. of blood in the short space of forty minutes made no impression upon the disease, though the energy of the heart was enfeebled and a pulse "full, hard and rapid" made small, feeble and thready. Nor did the calomel and opium, upon which I with so much confidence rely in abdominal inflammation, appear to check the alarming progress of the complaint. It may be observed, that though the loss of blood did not effect any immediate change in the disease, yet the ultimate good effects realized were in all probability owing to it. To this I would remark, that venesection, especially when it is necessary to push it a considerable extent, generally, if not always procures the patient, when the blood runs *pleno rivo*, considerable relief even before the orifice is closed, and we are invariably directed by clinical writers, to let the blood run in a bold stream until there is a manifest reduction in the intensity of the pain. In this case we detracted blood until a "full, hard and rapid" pulse was made "small, feeble and thready" in its character; we carried it indeed as far as the symptoms would justify, and instead of giving any check to the disease it progressed heedless of the remedies that had been used.

The remark I made, when commenting on case 6th, with regard to the lancet in peritoneal inflammation, and which is the result of actual experience, clearly shows that the lancet, though a useful auxiliary, is by no means absolutely essential in the treatment. But a few days since I treated two cases of a good deal of violence, by calomel and opium exclusively and with a success entirely satisfactory. They are not, however, the only grounds upon which I predicate the remark, as I could bring to my support others equally conclusive. Since therefore we may dispense with the lancet, I think we may fairly conclude, when we take into consideration the total absence of

relief from its employment, that it had no agency in subduing the inflammation or in saving my patient's life. In concluding our comments upon this case, we would only direct the attention of the reader to the article that appeared so supremely useful. The entire failure of the remedies that had been used, and the prompt and decided relief procured by a large dose of opium, suffer us to come to no other inference, than that the narcotic was the article that arrested the progress of the disease. Does not this case disclose an important and plain practical suggestion? Is it not probable that, opium alone, would subdue abdominal inflammation? And if it would, is it not certain, that it would do it more certainly, more quickly, and may I not say more safely than any other remedy? My determination, however paradoxical it may appear, is to try its exclusive agency in the first case of moderate intensity that comes under my notice, so that if I should be disappointed in my expectations, I may be able to retrace my steps without compromising the interest of my patient.

Case IX. *Enteritis.* Late in last April, I visited Mr. A. a merchant of this place and an Irishman by birth; aet 38; sanguine temperament; constitution much impaired by the intemperate use of ardent spirits. Seized the 29th with chill, which was followed regularly by the stages of a paroxysm of intermittent fever. In the morning he took an emetico-cathartic which acted extremely well *sursum ac deorsum*, after which the sulphate of quinine was given. The paroxysm returned at 1 P. M. and lasted about three hours. The quinine was again administered after its termination. Early on the morning of the 30th, he took a very large dose which he thought made him worse, and I was sent for. I saw him at 7 o'clock with tense full pulse; sallow bilious skin; and considerable abdominal pain which was much increased on pressure; his stomach was irritable, having rejected the quinine about an hour after it was taken; his bowels morbidly active, discharging copious watery stools. Bled him ℥xvi; and gave him half a drachm of calomel and two grains of opium. The same dose was repeated at 10 A. M. and at 2 P. M. which succeeded in subduing the disease.

The next day castor-oil was exhibited which completed the cure.

Remarks. While this case merits attention, on acount of the success of the remedies that were employed, we would invite the attention of the reader to the change which took place in its character. The disease being well defined at its onset, and throughout the first day, we could involve ourselves in no error, as to its real nature; and no other probable cause presenting, together with the circumstance of the pain ensuing immediately after taking a large dose of the quinine, we are disposed to attribute to it the production of enteritis. If my conjecture be well founded, this is an effect of the article, that has never been suspected before, and should merit the attention of practitioners, for if there is a peculiarity in some individuals, which enables it to produce in them inflammation of the stomach or bowels, the symptoms indicating this peculiarity should if possible be discovered, as much and indeed irreparable injury may be done before we are aware of it. That there is a peculiarity of this kind in some patients, I feel persuaded; for I have too frequently given the article without producing any such effect, to believe that it obtains generally. Are we not here put in possession of an important argument against the doctrine of *gastro-enterite*, as taught by Broussais? Does it not appear reasonable to suppose, that if intermittent fever consists in nothing more than inflammation or even irritation of the stomach and bowels, that quinine would never succeed in removing it, since in the case under consideration, there were two distinct paroxysms and, it was afterwards converted into enteritis, when to all appearance no other cause except the quinine had any agency in producing it? But this remedy is almost universally successful, and never did in any other instance as far as my knowledge extends excite intestinal inflammation. We are therefore warranted, so far as a single fact bears upon the point, in refusing to subscribe to the doctrine taught by Broussais.

While speaking of quinine, it may not be improper to remark, that in a case which recently fell under my notice, there was such a marked coincidence between the exhibition of this

medicine and the recurrence of the chill, that I feel convinced instead of preventing, as was expected, it was the cause of its return. The case was originally one of bilious fever, and of a very high and obstinate grade of action. It was the tenth day of his disease when I saw him, and his life was despaired of; active mercurial and aloetic cathartics, however, rescued him from danger in the course of six days. The disease, before remittent, now assumed an irregular intermittent form; there was no perfect paroxysm but an occasional cold sensation would run over his back; his feet and hands would become cool and his features looked shrunk. The quinine was given to prevent this, but in the evening of the day when it was used, he had a complete and perfect paroxysm of ague and fever; not a chill, but what the vulgar call a *shake* and one too very violent in its character. Three days elapsed when we thought that the quinine was again indicated, and the same effect was produced; and this was the result upon the third and fourth trial when the remarkable coincidence arrested the attention of our (Dr. Booth and myself) patient's wife, who objected to his taking any more of it. On those days that the quinine was not exhibited, he had the sensation to which we have alluded, and as certainly as he took it, so certainly he had a "*shake.*" The arsenical solution finally succeeded.

Case X. *Bilious Fever.* In the autumn of 1822, I visited a Boatman who had a few days previously left Louisville, where a bad form of bilious fever was then prevailing. Four days before I saw him he was attacked, and being a stranger destitute of friends and money, he was neglected by those into whose hands he accidentally fell. When I saw him I considered him almost moribund; he had low muttering delirium; very black tongue with sordes about the teeth; he picked the bed-cloths; deglutition embarrassed; urine discharged involuntarily, but his skin was dry and hot.

I considered the case hopeless, and being among the first cases that occurred to me after I commenced practice, I without much reflection, and with still less judgement, at least according to the principles upon which I had been taught at that time

to treat such cases, commenced precisely as I would have done, had I seen the patient at the onset of the disease. The only circumstance that seemed to occupy my mind, was his never having been purged since his attack, and by this perhaps I was too exclusively governed. I gave him at one dose fifteen grains of calomel, eight of aloes and one of gamboge directing it to be repeated every three hours until he had taken four portions. He was also blistered upon the epigastrium, wrists and ankles.

I left him about 9 o'clock at night; and in a ride of seven miles no man could have suffered more reproaches of conscience than I did. Reconsidering the case, I became totally dissatisfied with the course I had pursued; I at once concluded that I should have treated him by stimulants exclusively. More than once I determined to return, and countermand my orders, but my professional pride and self-love overcame me, and persuaded me that death was inevitable under any treatment. Day had scarcely dawned when I was again at the bed-side of my patient, and my surprise and gratification can be more readily conceived than described when I found him not only alive, but in a condition which the most sanguine hopes could not have expected from any remedy. The fourth and last dose had just been given when I arrived; the second relaxed his bowels, and in the course of the night five large, black, and excessively offensive alvine discharges had come from him. His delirium had disappeared and left a dull calm behind; his pulse was fuller, larger, and had so much tension as almost to induce me to use the lancet; the skin was soft and perspirable, and he had not picked the bed-cloths since 5 o'clock. His bowels continued to act with tolerable freedom till 3 P. M. when I gave him 15 grs. of calomel and as many of rhubarb; for although, it was in violent opposition to my preconceived notions, I could not refrain from continuing the purgative plan, when the signal benefit already derived, so clearly and emphatically pointed to its propriety. The next day I visited him and found him still improving, the cathartic having procured him three full, black stools. During the three succeeding days the same purgative was exhibited

pro re nata, at the expiration of which time his evacuations assumed their healthy aspect and odour. From this time I considered him convalescing and in a few days he left the country.

Remarks.—It is strange, but not the less true, that I have not had boldness enough to apply the important practical suggestions presented by this case to others in a nearly similar situation, and which have generally been unfortunate. For when I have been summoned to the treatment of the typhoid stage of autumnal bilious fever, I have been too apt, in conformity to vulgar as well as professional prejudice, to rely upon stimulants. These have usually failed, but when they were employed, I would advise the reader, that it was almost uniformly in second hand or consultation cases; for when I have the management of the disease from its onset, my treatment is so very active and generally successful, that I have nearly always warded off the sinking stage. And here let me remark, that I rely almost exclusively upon the detraction of blood, and large and frequently repeated doses of calomel, and have had a success which, if I shall continue to realize, will induce me to be satisfied, never to look for, or expect to be in possession of a treatment more efficient.

This case has been drawn from my *Case Book*, by the recent and very able memoir of Professor Cooke on "*Autumnal Diseases*," from which it appears, that he has been in the habit of pursuing a course in similar cases not materially unlike that which I accidentally and fortunately adopted. This practice has had in his hands a success totally unparallelled in the treatment of bilious fever. In the correctness of his practical views, the above case, together with two others, that have come under my notice since the publication of his essay, induces me to have the most entire confidence.

To my friend Doctor John C. Richardson of Lexington, I gave the history of this case a few weeks after its occurrence, in a conversation on the treatment of fever, and doubt not that he will immediately recognize its conspicuous features.

October 4th. 1828.

Case XI. *Apoplexy.* Just as I was about closing this communication, a singular case of apoplexy fell under my notice. About a week since I was called to an old negro of Judge Moseley, and found her labouring under bilious fever. She was æt. 69; very corpulent; short neck; large head; subject occasionally to paralytic shaking of the head and particularly of the lower jaw. The administration of medicine was defered three days on account of her great antipathy to it. When I saw her I thought the prospect discouraging, but the use of the lancet and frequent mercurial and aloetic cathartics in the course of four days subdued the fever, and I flattered myself that she would recover. On the fifth she had sufficient appetite; her tongue was clean and moist; her skin pliant and perspirable and she complained of no uncomfortable sensation in any part. This state lasted until about 2 o'clock P. M. when all at once and without giving any premonitory symptom of its approach, stertorous breathing was manifested. I was summoned and found her, about four hours after this symptom made its appearance, labouring under what I conceived to be apoplexy. Her breathing, as has already been observed, was stertorous; her face was swelled; the veins of the neck, head and particularly of the temples were turgid; her eyes appeared to protrude between the lids; the pulse was slow, tolerably regular and vigorous; the lower jaw ceased to shake; the extremities remained in whatever posture they were placed, and severe pricking could not excite in them the least motion; strong sternutatories made no impression upon the olfactory nerves, but the sensibility of the auditory apparatus, as well as the functions of the brain, appeared unimpaired. When addressed, she seemed to hear nearly, if not quite as well as ever, and although she could not speak, answers were returned by certain movements of the head, that were quite intelligible, and clearly indicated that she understood what was said to her. The remedies usually employed in apoplexy were resorted to without effect; she died about twenty-four hours after the attack.

Remarks. Apoplexy is defined by the accomplished Dr. Good "Mental and corporeal torpitude, with stertorous sleep." The

case here recorded does not correspond with his definition in two apparently essential particulars. Instead of intellectual torpor, there existed unequivocally perfect and unimpaired clearness of the mental powers, and though torpor seized upon the corporeal faculties generally, the head semed to retain considerable motive power. The complete consciousness, as presented in this case, exhibited an anomaly of a very extraordinary character, and has but a single parrallel, in the annals of clinical observation. A case in some respects similar, but more singular in its general character, is recorded in one of the Edinburgh Journals by Mr. Abraham of Worcester. This gentleman informs us, that in his case the power of thinking was not disturbed, and the patient retained the faculty of speech to a considerable extent. Though he seemed "*entirely senseless*" he understood perfectly the purport of the questions that were propounded to him, and observed in a low whispering tone, that he was conscious of all that was going on about him.

The Medico-Chirurgical Review speaks of his case and its author in the following language. "We take him (the author) to be very young, otherwise he would have perceived that his patient's malady was seated in the *stomach and not in the brain.*" We see no absolute necessity for joining the Reviewer in this inference, for the case we have recorded is analogous in its most conspicuous feature to that which fell under the observation of Mr. Abraham; and we are persuaded from an accurate investigation of this case that there could be no repletion of the stomach or offensive matter of any kind in that organ. All the aliment she had used was a few spoonfuls of soup, and even this she had not touched since 7 o'clock A. M. Moreover the active depletion which had been sustained during the four preceding days would lead to the inference that it ought to have shielded her for the present, from all immediate danger.

October 9th. 1828.

ART. III.—*Annual Address delivered before the Medical Society of the State of New-York, February* 4, 1829. By T. Romeyn Beck, M. D. President of the Society.

GENTLEMEN OF THE SOCIETY,

SINCE we last closed our annual deliberations, events have occurred to remind us, that as well in our corporate as in our individual capacity, we are not exempted from the common lot of man. An aged and respected practitioner, one of the early Presidents of this Society, has during the past year, yielded to the inevitable stroke of fate.* Nor ought an allusion to his name to be omitted, under whose magistracy we held our last meeting. The friend and eulogist of our profession—the scholar and the statesman—the pride of his native state, has suddenly been removed.

It is a "taming thought to human pride," that although death destroys those whose existence is deemed most necessary to the common welfare, yet the train of events proceeds on with a steady march. It is no less evident that in the present æra of advancement in knowledge, no minor obstacle can be found which will permanently impede its course. In all the schemes for the melioration of the condition of man,—for his growth in science —his progress in morality and religion, the loss of one master-spirit is early compensated by the acquisition of many ardent votaries. We feel this in our own profession. The general diffusion of information brings numerous labourers into the common harvest—and emulation is excited as well by the examples of the present day, as by a retrospect of the past. There is no branch of medicine—either strictly belonging to its numerous departments, or ministering collaterally to their improvement, which is not industriously and successfully cultivated.

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*Hon. William Wilson, M. D. of Columbia county, President of the State Medical Society, during the year 1813.

It is impossible to survey this effort, or its results, without a sensation of pleasure, or without a wish that its value may be generally understood. That there are impediments to this, is only to confess that human nature is imperfect and wayward. But if we examine more closely into the causes of the indifference or ignorance manifested by the greater part of mankind, we shall, I apprehend, find it mainly owing to the negligence of our own profession. The importance of the subject—its extensive and interesting bearings on human happiness—its magnitude, even as a branch of scientific study, are not sufficiently presented in relief before those who can judge with discrimination, or appreciate with accuracy. All proper occasions should be embraced to manifest to the community what is doing to remove the charge of uncertainty in medicine, and to illustrate the discoveries which the talent of the present age is bringing forward. I avail myself of the opportunity, at the conclusion of my term of office, to dwell for a short time on this subject in its iteresting relations.

The first illustration I would offer of a great IMPROVEMENT IN MEDICINE, is the *distrust which is obtaining against general theories.* There was a period when every physician ranged himself under the banner of some great name—adopted his doctrines, and obeyed the precepts which he inculcated. Many of the celebrated individuals to whom I refer, were indeed master spirits; whose lives cannot be read without admiration, and whose eloquence and learning won them willing votaries. Each in his turn, in various ages, down to our own days, fondly imagined that he had reared the imperishable monument, which neither time, nor human industry could overthrow—each imagined that he at least, had developed the laws of life—discovered their mysterious workings, and elucidated the changes that disease produces in them. But like material monuments, they have slowly crumbled, or been violently overthrown, and we search among the ruins for some precious relic that may have been forgotten in the destruction. Such they all contain. There is no theory ever yet promulgated, which is not based on some portion of positive truth. Indeed it could hardly have been

otherwise. The founders were men of vigorous intellect and of keen observation. They drew from their own stores, and were only wanting in that universality of knowledge which is essential to the formation of a perfect system—They seized on some prominent phænomenon, and after examining it with skill and acuteness, made it the groundwork of their superstructure.

In the devotion so natural towards a favourite opinion, other facts no less striking, were either unheeded or diminished in value to their mental view. Thus the basis was frequently narrow and contracted, but not, as some are fond of asserting, imaginary.

A subsidiary but important advantage has also accrued from the successive promulgation of these theories. The disciple, animated with an enthusiastic belief in the doctrines of his preceptor; warmed by the eloquence of his prelections, or excited by the attacks of his opponents, applies himself to the invention of new arguments, or the detection of fallacious objections. In such a contest, with much that is superfluous, and often something that is offensive, additional facts are notwithstanding elicited, and the value of old ones is better understood and more correctly applied. A rapid accumulation is thus produced, of the mass of knowledge, for which otherwise, years of desultory effort would have been necessary. The utility of this has been noticed in other sciences. In geology, in particular, the promulgation of various theories, and the ardour with which they have been impugned or defended, certainly are among the principal causes of the numerous and most interesting data, of which it now boasts.

But notwithstanding this compensating benefit, the main objection which I have made remains, and it applies with peculiar force to these doctrines which are offered as explanatory of the phænomena of fever. This disease, in its various forms and numerous combinations, is a fertile source of investigation and most deservedly so—for it embraces a large proportion of the "ills to which flesh is heir." Fevers strictly so called—the local inflammations—many forms of eruptive disease, and of hæmorrhage, besides a numerous list usually included under

the head of chronic diseases, owe much of their character, as well as severity, to the symptoms usually denominated febrile. —To explain these in their order, and to account for the anomalies they so frequently present, has been deemed a fit employment for the wisest and most intellectual of our brethren in successive ages. Their want of success may be read in the partial prevalence of a modern theory, of which, while I would desire to speak of it with respect, I must notwithstanding remark, that like its predecessors, it appears too partial and confined in its application. Its popularity amongst a portion of the medical profession in France, and the favourable reception it has met with in some parts of our own country, is not, I apprehend, any invalidation of what I have already said concerning the *general* distrust that is entertained. Due praise may be given for the improvements in pathological research which it encourages, (and which I shall hereafter more particularly notice,) and for the caution it has taught against pushing too far the use of some of our most active and valuable medicines; but its leading propositions cannot become the creed of the profession. A similar remark will apply to the opinions on the same disease, promulgated in England, by a talented individual. They both run counter to that course of practice, which, although requiring nice adjustment, has yet on the whole, been found, by those who have had the most extensive experience, the most beneficial in checking or removing disease. They necessarily lead—the one often to too active treatment—the other to an inefficient one—and though in the hands of their authors, these difficulties may not occur, yet in those of their followers, the results, if we may judge from all former experience, would appear to be inevitable. I am aware of the delicate ground which I occupy in making these remarks, and that I may expose myself to the charge of rash and unfounded censure; but the present is not the place for discussing the merits of these doctrines, and I have only aimed to announce and elucidate a fact, which I believe you will agree with me, is correct, viz. that there is a great repugnance among the profession to embrace general theories, and to shape their practice in

strict conformity to them, and that this repugnance is constantly increasing. Whether this is owing to ignorance or perverseness, is a secondary point of inquiry. I for one am free in ascribing it to the more general diffusion of knowledge—to a candid but decisive application of the experience of the past—and to an increasing desire to study diseases distinctly with reference to their appropriate, as well as ever-varying symptoms—and the changes that are thus produced.

This leads me to the notice of a second feature in the present history of medicine, illustrative of its improvement, viz. *the advancement in Pathological Research.*

If there be a single individual in later times to whom the honour of exciting and directing to this particular branch, is pre-eminently due, it is the illustrious BICHAT. His life was closed at the early age of thirty-three years, but that short span was sufficient for him to produce his volumes in General and Descriptive Anatomy, his Treatise on the Membranes, and his Essay on Life and Death. And these works were the result of numerous and varied experiments and observations—of laborious and unwearied employment in the dissecting room—and finally, of the application of rare intellectual powers to their generalization and arrangement. The division of the body by him into its elementary textures—and the inquiry into their distinct properties, with the changes they undergo from disease—form the basis of discoveries, of which we cannot yet appreciate the extent, or the value.

Since his lamented death, the path of inquiry has been thronged by his votaries and admirers. In France, pathological anatomy is pursued by hundreds of ardent and active minds, and in England, with each succeeding year, it is evident that the subject is gaining and commanding consequence. In this nation, with obstacles peculiar, and in some instances almost insurmountable, some additions have been made to the general fund. The result is cheering to the man of science, as well as to the philanthropist. Knowledge is advanced, and human suffering is mitigated or removed. Look at the practical operation of this pursuit. An individual after struggling with

disease which resists all medical skill, sinks under its effects. The symptoms have been narrowly watched by his attendant, and he has endeavoured to apply appropriate remedies. But occasionally some appearances have been noticed, the cause of which he cannot explain. His medical agents also do not produce their usual effect. What are the morbid changes that have caused this? If the lifeless body be consigned to its mother earth without examination, can any information, except of a mere negative kind, be drawn from the case, to be hereafter applied for the benefit of the living? Not so is the practice in the foreign countries which I have noticed. The view of the ravages of the disease illustrates the alterations which have taken place; and although they frequently appear rather as effects, than causes, yet sufficient is obvious to enable useful deductions to be formed, to explain many of the symptoms, and to furnish materials for reflection and improvement in future practice. Need I suggest that many diseases are yet imperfectly understood, and can only be elucidated by the light of this torch. How many have been thus developed by the labours of Morgagni, Baillie, and others. How many, even within the present century, have been successfully explored by Laennec and his compeers in France, by Bell and others in England. The nature of new or unknown diseases can thus alone be discovered. Allow me, in illustration, to refer to a case of mortality, the sudden announcement of which is feelingly remembered by every man in the community who was then living. If the death of our beloved and lamented WASHINGTON, was owing, as would seem from the brief account we have of his symptoms, to the disease now styled Laryngitis, where was the instruction to guide the practitioner? It was not until several years thereafter, that the sudden decease from it, of two distinguished physicians in London, led to pathological examination. Its nature became evident—its mortality was rendered less certain—and dangerous as it must ever prove, instances are even multiplying of recovery from its effects. And yet this disease must have occurred centuries ago, and who can tell how many lives might have been pro-

longed, had its character been thus specially marked, and investigated at an early period.

The tendency of these observations necessarily leads me to notice the importance of dissection. I am aware of the prejudices that must be encountered in discussing its necessity; but they must be overcome, if at all, by argument that shall shake their vehemence. They owe their origin in some respects to the best feelings of the human heart—while they are unquestionably heightened by the practice of disinterment. But it is because we would render this alternative unnecessary, that I venture to mention the subject.

That an intimate knowledge of the condition of the human system, both in health and in disease, is indispensable to the judicious application of curative means, would seem to be an axiom that requires only to be stated, in order to meet with universal credence. It is no less certain that the community must suffer from the consequences of ignorance. The truth of these remarks appears most strikingly in the department of surgery, although they will be no less conspicuous to him who will investigate, in that of medicine. In the language of the report made last summer to the English House of Commons, by a committee of its members, not professional, (a paper which should be in the hands of every intelligent citizen in our own country) "as the science of anatomy has improved, many operations formerly thought necessary have been altogether dispensed with; most of those retained, have been rendered more simple, and many new ones have been performed, to the saving of the lives of patients, which were formerly thought impossible. To neglect the practice of dissection would lead to the greatest aggravation of human misery; since anatomy, if not learned by that practice, must be learned by mangling the living. Though all classes are deeply interested in affording protection to the study of anatomy, yet the poor and middle classes are most so: they will be the most benefitted by promoting it and the principal sufferers by discouraging it. The rich when they require professional assistance, can afford to employ those who have acquired the reputation of practising successfully. It is

on the poor, that the inexperienced commence their practice."

But it is not the poor only that are thus to be benefitted by the pursuit of anatomy. There is another class of the community whose condition must excite all the best feelings of our nature—the victims of the elements—at a distance from their home—deprived of the blessings of domestic life, and yet proudly maintaining the honour and glory of their native land. Our gallant soldiers and sailors, in time of war, have no solace when the hour of danger approaches—no hope amidst the storm of battle, that they shall escape from the consequences of dangerous wounds, except in the skill of their medical attendants. The records of the armies and navies of foreign countries, and particularly of England, where the materials for comparison are most copious, attest how much of human life has been thus preserved, and how great has been the diminution of mortality, when compared with former years.

A word or two is necessary in defence of those who pursue these studies. All will grant that they would not have been selected, except from a high sense of duty. It requires some lofty incitement—some moral courage, to be thus employed. The mysterious change which death induces, is alone sufficient to startle the most buoyant spirit; but with this, the pathologist must familiarize himself. He proceeds to his high office at the risk of health—often indeed of existence. I appeal to your reading, in confirmation of the truth of my assertion, that for several years, scarcely a medical journal has arrived from abroad, which does not contain an account of some individual, who has either met with irreparable injury to his constitution, or has lost his life, from accidents occurring during dissection. Instances of a similar nature have happened in our own country—And apart from the honourable ambition of acquiring some fame, what can be the object of this untiring and hazardous labour? Is it not to ascertain what has been the cause of the mortality, and whether its seat and nature are under the power of human skill? Surely, under the obstacles which I have noticed, a triumphant proof is given of the desire of improvement in our profession.

I will only notice a third circumstance indicative of my main proposition—viz. *a most remarkable improvement in the composition and administration of remedies.*

The slow progress of the materia medica in former times, is sufficiently explained by the fact that remedies are applied to a system, that we are scarcely ever certain, is constituted exactly alike in any two individuals. Apart then from the general operation of medicines, and even these are modified by the presence of different diseases, numerous observations are required to establish their respective characters. In addition to this, it was so much the fashion in former times, to unite several in one prescription, that it would seem almost impossible to designate the distinct effects of each. The imagination also often aids in diversifying these results. Some patients, buoyed up by an implicit confidence in a particular substance, assist as it were, its effects, while others take it in doubt and fear, and thus deprive it of half its efficacy. Another and most serious impediment to the advancement of the materia medica, has been the number of medicines which encumber our pharmacopœias and dispensatories.

Some of these difficulties are from their nature insurmountable—since they attach to the nature of the human system, and are inseparable from it. There are others, which our own times are about removing. It has been discovered, that many medicines, in whose efficacy all agreed, but whose effects were occasionally uncertain, are of a compound nature—that, along with their curative constituents, they contain useless or noxious ingredients. We are indebted for this knowledge to modern chemistry—a science eminently practical in its details, and promising at no distant day to remodel its sister branch. I cannot adduce more striking examples of the fruit of this analysis, than the discoveries that have been made concerning bark and opium.

The first of these, the Cinchona or Peruvian bark, has for many years been known as a powerful febrifuge. It was ascertained that there were several species of trees, which furnished it, and experiments soon showed that their qualities were vari-

ous. From time to time a change took place in the popularity of the different kinds—but the difference in quality of the imported article, and the repeated disappointments experienced in its administration, either from loading the stomach, or causing other results destructive of its anti-febrile powers—all tended to impair its general reputation, and there was a period, when another remedy, but which all would agree in omitting if its place could be supplied, was substituted. Chemists however have analyzed the bark, and found that, instead of being homogeneous, it consists of eight or nine ingredients. They have separated the important constituent (the quinine,) and in its distinct form, it is now to be purchased of every druggist. Of its general success in the removal of intermittents, every physician can speak.

Opium also is a remedy that all united in commending, when judiciously administered. Its powers in mitigating pain and in soothing the wearied mind and diseased body, entitle it to a high rank among medicinal agents. Yet its operation has occasionally been unkind; and often when we are most desirous of obtaining its anodyne effects, a dread of its attendant consequences has restrained us from recommending it. Substitutes, comparatively inefficient, were necessarily employed. But about twenty years since, the compound nature of opium began to attract the attention of chemists, and from the experiments of Sertuerner, Robiquet, Pelletier and others, it seems now established that it consists of (besides several unimportant ingredients) three principles—morphia—narcotine, and meconic acid. The first of these produces the soothing effect—without the feverish excitement, heat and head ache, which so frequently accompany the employment of the drug, and which from experiments seem referable to the second substance I have mentioned. The third does not appear to produce any effect on the human system. If morphia be united to an acid, its stimulating properties are apparently still further counteracted.

These are surely important improvements, and the more so, as they allow its use in states of disease, where it must otherwise have been interdicted.

But it is not only to the investigation of the nature of old remedies, and of which I might continue to adduce numerous examples, that the intellect and labour of the professsion are directed. New therapeutic agents are constantly developing. Iodine was unknown in 1811. A manufacturer at Paris observed, that in preparing carbonate of soda from the ashes of sea-weeds, the residual liquor powerfully corroded metallic vessels. He inquired into the cause of the corrosion, and the result was the discovery of this substance—a simple, undecomposable body, so far as our knowledge extends. The next step was to ascertain in what bodies it exists—and among these, sea-weeds, the kelp prepared from them, sponges, and some marine animals, were found to furnish it in the greatest quantity. A physician, recollecting that burnt sponge had acquired some local reputation in the treatment of goitrous affections, embraced the idea, that its virtue might have originated from the iodine it contained. He prescribed it in these diseases, and found its effects so powerful, that it was necessary to alter the form of administration. Yet his success and that of others have been great. Its use has been extended to the diseases of other glands. The result is still under judgment—but all agree, that an agent has been discovered more direct in its operation, than any yet known, on those maladies which minister to the production of our most common and fatal diseases.

Are these examples which I have adduced satisfactory in showing the exertions of the profession to improve the nature of remedies? Are they sufficiently illustrative of its desire to increase them, so far as sober judgment and prudence will warrant? If so, why should others, who are not of the profession, and who cannot, even in common charity, be supposed to have the necessary preliminary knowledge, either of medicines or of the body, be permitted to employ their supposed curative agents? It has been replied, that the use of vegetable remedies alone are to be permitted, and the usual argument advanced, that if they do no good—they will do no harm. But I would ask, whether the effects and the extent of vegetable and mineral medicines when thus placed in opposition to each other,

are sufficiently understood. The supposed deleterious substances of the former class, which are most commonly used, are arsenic and mercury. They have been known for centuries as poisonous substances when given in considerable quantities. The child hears and remembers this; and the man, long before he enters on the practice of medicine, has his mind deeply impressed with the necessity of administering them in small doses, and then only with caution, and after mature reflection—they are so given. Arsenic combined with potash has been used for upwards of forty years, with great success in the treatment of agues and other diseases. But it was not from choice. It was because the bark failed in numerous instances—because its quality was often bad, and its effects even positively injurious. For the reasons already stated, the use of quinine is rapidly diminishing the employment of the mineral.

As to mercury, I will only say, that if there be a single drug, that in human hands has proved a most distinguished curative means, it is this. That its effects may be occasionally uncertain or injurious, is only to repeat, that there are peculiarities of constiution for which the wisest cannot be prepared—or that there is a malignancy in some diseases which no human effort can counteract. But who will deny its positive utility? Look at the thousands whom, not only in tropical climates, but in our own country, it has saved from that fell destroyer of our race—fever. Look at the numerous inflammations it has checked or dissipated. Look at the tens of thousands whom it has saved from the consequences of vice.

A similar answer will apply, if we notice the remaining medicines. Their noxious effects (if any) are known—they are given under the high responsibility of this knowledge, and they are selected, notwithstanding this, because their effects are more certain, and their operation as kind, as those of vegetable substances.

But it would seem to be intimated that mineral medicines are incongruous to the human system, and a certain mysterious affinity has been imagined between it and vegetable remedies. The latter, it is asserted, are milder in their operation; produce

healing effects more certainly, and are not injurious, like calomel, in their consequences. Are these assertions founded in a knowledge of the human body, and do the authors of the first idea know of what it is composed? Have they ever heard that a large proportion of the solid parts of the body are a mineral substance—the phosphate of lime? Do they know that iron enters into the composition of the blood?

But this is not all. Allow me to throw a rapid glance over these mild vegetable remedies—these substances that any man after a day's reading may administer. I have already enumerated two of them, opium and iodine. Opium has been raised in this state. It may be thus an indigenous product. Iodine may hereafter be extracted from the sea-weeds on your coast. Now a grain of narcotine extracted from the first, has killed a dog, and a few grains of the last taken daily for a short time, has destroyed a human being. Do you know of a substance which indicates its power over life by lessening the action of the heart with great rapidity—which reduces the rapid beat of the consumptive patient to comparative slowness? It is foxglove, a vegetable cultivated in many of our gardens. Are you acquainted with vegetables which will produce raving madness and convulsions—which inflame and destroy the texture of the stomach? The stramonium, cicuta, and wild parsnip will do this. They too are indigenous—they grow in our streets or lanes—or abound in our marshes. There is a substance which in small quantities convulses the palsied arm of the bed-ridden—the limb that has hung dead for years by his side. It is strychnine, a vegetable product. But to crown all this—what is the most concentrated and deadly poison with which we are acquainted—which will destroy all species of life, from the snail up to man, with the rapidity of lightning—which has struck down the suicide in death, before he could finish the fatal draught? It is prussic acid—a vegetable product, that can be distilled from the leaves and kernels of the peach, or from the laurels that abound throughout our state.

And yet, after this enumeration, we have only gone through a portion of the noxious vegetable substances. Let not this

statement startle any of my hearers. It may be laid down as an axiom, generally true, that with the exception of some remedies in the classes of astringents and tonics, a vegetable is valuable as a medicine, in proportion as it is noxious in large quantities. In every botanical work, you find the remark, that those which are not possessed of some acrid qualities, are inefficient in their operations on the human system.

The question then presents itself to the mind of every enlightened man, whether those who have received an appropriate education; who have the stores of accumulated knowledge opened to them, are the safest persons to be entrusted with the care of the health of the community, or whether they shall be put on a par with such as claim instinct—or self-inspiration as their guides. Can these soldiers of Cadmus be expected to bring any useful spoil into the camp? Look at every modern work on the materia medica. We now know the nature of almost every secret remedy—Chemistry furnishes us with the means of analysis, and almost without an exception, they are the cast-off prescriptions of the older physicians—neglected, because more eligible substitutes have been obtained—or abandoned, because their constituents were found uncertain or dangerous. That man will render a service to the community, who will furnish our public prints with the receipts for compounding these vaunted catholicons.

I rejoice, that as your representative, I have been permitted to bear my testimony on this subject. I do not feel that the profession are to be the greatest sufferers. It will be the community—the country. The three professions at this day furnish the greatest proportion of educated men. Education is essential to the stability of the republic. Will it then be attempted to break down one of these professions? If so—the young men who now crowd its avenues, will seek other means of support—the old will retire in disgust from the thankless contest. The study of the collateral sciences, which has ministered so largely to the advancement of medicine, and which is beginning to give character to the nation, will cease. *The era of improvement in medicine will have passed away,* and the

encomium of Sir William Temple, quoted, and sanctioned, and applauded, by DE WITT CLINTON, will no longer apply. "It is certain (says he) that the study of physic is not achieved in any eminent degree, without very great advancement in the sciences, so that whatever the profession is, the professors of it have been generally very much esteemed on that account, as well as of their own art, as the most learned men of their ages."

ART. IV.—*An Essay on Burns. By* Dr. LLOYD WARFIELD, *of Paris, Kentucky.*

A BURN is an injury done the human body by concentrated caloric. This agent, in different degrees of concentration, produces very different effects upon the human system. Diffused as it is in our atmosphere by the summer's sun, it acts as one of the natural stimuli of our organs, promoting the phenomena of life. Accumulated, as in red hot metals, boiling oils, soap, ley, syrup, water, and in substances in a state of combustion, it presents us with one of the most potent agents in the business of destroying that same system, which in a more diffused state it is instrumental in benefitting. Thus accumulated, according to the length of time it is applied, and the extent of opposing surface, it can produce all the phenomena of burns, from the mere redness of irritated skin, up to the death of the material exposed to its operation. Concentrated in red hot metals, it will produce in the same length of time, the worst of these phenomena more certainly, than if accumulated in water, or other thin fluids. Thus the power of burning is unequal in different substances, even when they are fully saturated with caloric. Bodies in a state of combustion, as wearing apparel, give out caloric very abundantly, and therefore burn severely.

Concentrated caloric, by its subtilty, is capable of affecting all the tissues of the part to which it is applied, and radiates from thence upon the general system. The phenomena thence resulting are local and constitutional. Those which are local are such as redness, vesications, hard black or soft yellowish eschars, insensibility, pain, and heat. The constitutional, are such as chilliness, muscular weakness, feeble circulation, restlessness, diminished animal heat, coma, delirium, tetanus, difficult breathing, and ultimately death. Some of these phenomena are common to all burns; others are peculiar or diagnostic, and can only belong to that class which they indicate. Those which are common, whether local or general, are such as redness, phlyctenæ, eschars, chilliness, coma, delirium, tetanus, and difficult breathing. Those that are diagnostic, are such as pain, insensibility, heat, which are local; and long continued chilliness, feeble circulation, permanently diminished and unequally diffused animal heat, muscular weakness, and restlessness, which are constitutional. So far as any natural and just division of burns is to be made from their phenomena, it is evident that those which are diagnostic should be made the basis of it. We think those diagnostic phenomena, with other considerations, justify us in dividing burns into two classes. For these phenomena alone, indicate two states into which the part injured and the general system may be thrown, by the operation of caloric. The first is that in which the vitality is injured, and both the part and the general system retain, and manifest, an abundant power to react. The second is that in which the vitality, both in the part and the general system, is greatly exhausted, and of course but very little power of reaction remains.

We are not unaware that a division of burns has been made, without a reference to either the state of the general system, the modus operandi of caloric, or the known methods of treating burns; but solely in regard to what we have denominated their common local phenomena. Thus we have three degrees of burns as advocated by Baron Boyer, in his treatise on Surgery. The first degree is characterized by redness, and amounts to

moderate irritation in the part burnt. The second is distinguished by phlyctenæ, in which irritation rises to a higher pitch. The third is indicated by eschars, in which the part is disorganized. We cannot but esteem this division fallacious when we consider, that any one case of burns may exhibit all these phenomena, as indeed happens in most bad burns; and also when we reflect, that they may occur, indifferently, in cases in which the power of reaction is retained, and in those in which it is nearly extinguished or very feeble, both in the part and the general system. It is not only fallacious, but unsatisfactory and imperfect; because the same mode of treatment is applicable to all the degrees. For example, in the third degree, which may and does include all the others, the antiphlogistic plan is just as suitable as in the first degree; because the one may be attended with as much power of reaction as the other. Let any one examine Baron Boyer's prescriptions for these degrees of burns; and he will find but little if any difference in them. It is known that the same treatment is not applicable to all burns, one case requiring the stimulating plan, and another the antiphlogistic. Besides all this, Boyer admits this division to be more in definition than in fact. From these various considerations we think ourselves justifiable in departing from that established division, and in endeavouring to form one upon more rational principles.

We have said that the division into two classes, having a reference to the two states into which the parts and the general system may be thrown by the operation of caloric, is supported by the phenomena of burns. We also say that it is supported by the modus operandi of caloric, and the different methods of treating those injuries.

Some of the phenomena of burns are common to all such accidents, and from them no division can be established. We might give some examples of this: for instance, boiling oil may be applied to the hand long enough to disorganize the skin and cellular substance, yet the power of reaction is retained both in the part and general system, and the person soon recovers from the injury. The same boiling oil may be applied (so as to pro-

duce the same phenomena) to the whole leg: the power of reaction is exhausted, both in the part and the general system, and the patient dies. Again, hot water may be applied to the foot, so as to raise the skin, and inasmuch as the power of reaction is not affected, but little inconvenience is experienced; but hot water applied (so as to produce the same effect) over the region of the stomach, shall exhaust the power of reaction, and the patient dies. These phenomena can establish no division, for they are present indifferently in those burns which do well from the first, and those which speedily result in death.

The diagnostic phenomena establish incontrovertibly the two states of the system upon which is founded the division of burns into two classes. They are confined exclusively to those cases belonging to the class which they indicate. For example, severe local pain of long continuance, and heat, and undisturbed vital functions, characterize the first class, and belong exclusively to such cases of burns. They cannot attend a burn of the second class, because such phenomena can only exist when the vitality of the part and the general system is unimpaired. Again, local insensibility, coldness, and feeble circulation, with permanently diminished and unequally diffused animal heat, long continued and severe chilliness, and muscular weakness, characterize the second class, and belong exclusively to that class. They cannot attend in any degree a burn of the first class, because they are the phenomena which ensue upon exhausted vitality, both in the part and the general system. Hence the operation of caloric, in burns, is attended with these two states of the system, because burns are attended with the phenomena which can alone arise from these two states. The phenomena of burns then support the division of those accidents into two classes.

The modus operandi of caloric authorizes the division which we contend for. If we look upon a part burnt, we shall perhaps find some portion of it disorganized, killed, and another portion, though greatly injured, alive. Caloric has acted upon the portion disorganized as a burning, destroying agent, but upon the portion alive it has acted as a potent stimulus. Inasmuch as

that portion which is disorganized, can never be reanimated by any power of which we are possessed, caloric viewed as a burning agent, can claim no consideration from us. But inasmuch as the portion which is alive is susceptible of restoration, it is to that that our efforts are directed; it is to that we are to look for the phenomena indicating the one or the other of our classes; and caloric, therefore, as a stimulant alone, demands our consideration.

As a stimulant, like all others, it acts with different degrees of force, according to its different degrees of concentration, and time of application. Like many other potent stimulants, it is capable of influencing the nerves and blood vessels of the part, and even of the whole system, at the same time. In one degree it appears to arouse them both, acting as a pure stimulant; in another, it appears to exhaust them both, acting as a sedative. Its operation as a pure stimulant, is perhaps well exemplified in the action of mustard seed, Spanish flies, &c. upon the parts to which they are applied. The phenomena resulting from either, are such as indicate abundant power of reaction, and are such as severe pain and heat in the part, and undisturbed vital functions.

In another degree, caloric seems to act as a sedative, exhausting in a very short time, the vitality of the nerves and arteries, and that not only of the part to which the fire is applied, but of the whole system. To its operation as a sedative, we are to look for its most terrible effects. As a sedative, it may be justly compared to any of the narcotic poisons, the agent of malignant fevers, gun-shot, &c. The phenomena resulting from the full and fair operation of these agents, are nearly alike. These phenomena follow in a short time after their application, and are such as local insensibility and coldness, feeble circulation, permanently diminished animal heat, muscular weakness, and long continued chilliness, or insensibility. Caloric, then, as a stimulant, possessing different degrees of force, according to concentration, &c. seems capable of reducing the human system, in the burns which it inflicts on it, to the two states upon which

our division is made. We therefore say, that the modus operandi of fire is consistent with, and supports that division.

The successful methods of treating burns, support the division of these injuries into two classes. There are essentially but two modes of treatment, and they are diametrically opposite. One is the stimulant, and the other the antiphlogistic. It is impossible that these two methods can be proper if all burns are alike. It is impossible that a stimulant plan can be indicated in a burn, in which the vitality of the part and the general system remains unimpaired. It is just as impossible, that the antiphlogistic plan can be indicated in a case, in which that vitality is nearly exhausted. In all diseases, stimulants are used to arouse a part whose powers are weakened, and antiphlogistics to lower those powers when too much excitement is present. To render these two modes of treatment justifiable, then, two states must obtain in burns, the one corresponding to the stimulant plan, and the other to the antiphlogistic. These two states are expressed in the division we have adopted, and hence we conclude that the successful modes of treating burns, support them.

It has been a common, and a very just remark, that no disease is treated with less conformity to theory than burns. This arises from the fact that all the theories on the subject have been bad, have in no wise embraced the grand phenomena of burns. The theories have been too partial, embracing a few of the phenomena; and hence has arisen that endless variety of prescription, and the exceedingly inconsistent and opposite nature of them. How else can we explain it, that one surgeon will direct stimulants, both internally and externally, to all burns? He must act upon an imperfect and confined theory; and that surgeon who recommends antiphlogistics to all burns, is in a similar dilemma. These things have been done by many of the teachers in medicine; what then may be expected of those who travel along the humbler walks of the profession? Dr. Dorsey (our distinguished countryman) has told us the truth on this subject, when in his Elements of Surgery, he says, "the method which has been found to succeed best by the surgeons of

this country, is to attend as in other cases to the state of the constitution." This is what has been too much overlooked, and is what we have endeavoured to draw the attention to in this paper.

It is proper to remark, that burns of the same class, may have a few or many parts injured, and may be of greater or less magnitude. In a burn of the first class, skin, muscle, cellular substance, tendon, bone, &c. may be all included, or only one, two, or three of them. The same thing may happen in a burn of the second class. Again, a burn of either class may embrace a very large surface, or a limited one. These circumstances are purely casual, and are owing to the longer or shorter application of the fire, and the manner of that application. They influence the magnitude of the patient's sufferings and misfortune, but can in no wise affect the immediate treatment, other than to urge that treatment more vigorously, which is suited to the class of burns to which the case belongs.

All burns are succeeded by more or less inflammation, in a longer or shorter time, in every instance in which the patient recovers. A burn in which the skin alone is involved, and that to a small extent, can never be followed by as much inflammation as that, in which skin, cellular substance, muscle, &c. are all embraced, and that to a great extent. Again, a burn of the first class, in which vitality is unimpaired, is followed by inflammation in a very short period after the injury, while a burn of the second, in which vitality is nearly exhausted, will not be succeeded by that phenomenon under three, four, or five days.

Inflammation always adds to burns phenomena which they did not possess before. They are such as sensible heat, swelling, throbbing, tension, dryness, &c. in the part, and a general febrile disturbance, indicated by a strong, active, quick, or a weak quick pulse, hot skin, thirst, white furred tongue, costive bowels, head-ache, &c. The grade of constitutional excitement is generally inflammatory; it is, though, sometimes irritative. The foregoing remarks should be kept constantly in view, when treating a burn; so soon as the system has recovered from the immediate effects of the fire, we may expect high inflam-

mation to come on, and if not arrested, it extends all the local injury, causing extensive vesication and mortification, besides endangering the patient's life from mere excessive constitutional fever. This inflammatory state of the burn continuing a longer or shorter time, is ultimately resolved, or ends in suppuration if the patient does well, or runs into mortification if he dies.

The ulcers which succeed burns, are treated upon the plain principles which govern our applications in ulcers from other causes, and therefore need not be noticed in this essay.

The treatment of burns now claims our attention. Every case of burn is properly subject to either the antiphlogistic or stimulant plan of treatment. The first method is applicable to burns of the first class, in which the power of reaction is unimpaired. The last method is adapted to the second class, in which the vital power is greatly exhausted.

The antiphlogistic plan consists in a medical treatment and regimen, whose immediate operation is to depress or lower the vital powers. Such a modus operandi is imperiously called for in the first class, because reaction ensues in a short period after the accident, and should be anticipated with a degree of promptitude and energy suited to the probable violence with which it will commence. The stimulant plan embraces a set of remedial agents and regimen, whose immediate operation is to elevate the vital powers. This modus operandi is indicated in the second class, because a state of extreme depression immediately follows the accident, and is not recovered from for hours and days. But it is evident that when reaction begins to manifest itself, this plan is no longer admissible, but must be substituted by the first. Whatever plan of treatment is instituted, it should be continued until the indications change. The antiphlogistic and stimulant remedies are each divisible into local and general. The local are such as are applied to the parts burnt, and consist in the dressings; the general are such as are directed to the constitution.

The local antiphlogistic remedies of most note are not possessed of the same sensible attributes or qualities. One class of them is distinguished and act by their coldness. These are such

as ice, very cold water, vegeto-mineral water, &c. &c. The modes of making such dressings, are pointed out by the different elementary writers, and to them reference can be made. Such dressings cannot be continued for any length of time; they are advised by those who make them, to be laid aside when the heat and pain of the burn have abated. I should not suppose them suitable to any but superficial burns. Another class of these remedies are characterized by their astringency. These are such as vinegar, alum water, various astringent vegetable decoctions, and perhaps brandy. Some writers are loud in the praise of these remedies; but my own experience does not permit me to advocate or oppose them. Another set of those remedies possess an emollient quality, and are such as poultices. These poultices may be warm or cold, and may be formed of a variety of substances. I think it proper to state, that moderately warm poultices applied immediately after a burn, and continued as long as the indication lasts, have been more decidedly beneficial, according to my experience, than any other dressings. They are better dressings, from their nature, than those we have mentioned. If the substance of which they are composed, be of a nature to undergo but little change, they may be kept on for a number of hours together, before they require a removal. This is an important end to be gained in dressing a burn, because every fresh dressing is attended with an increase of irritation in the burn. Not only this, but poultices, if sufficiently soft and thick, accommodate themselves well to the parts, and afford protection and ease, which thin dressings cannot so readily impart.

The poultices which I can recommend from experience, are two, one of the starch of wheat-flour, and the other of the slippery-elm bark. The first is prepared by dissolving starch in cold water, with which it mixes readily. Let this solution be stirred over a fire in a proper vessel until a sufficient coagulum is formed. It is now ready to be applied so soon as it cools sufficiently. My method is to spread it pretty thick on portions of linen cloth, then to cover its surface with linseed-oil, and while moderately warm to apply it to the immediate

surface of the burn. It need not be renewed under twenty-four hours, if care has been taken in making the poultice thick enough. It should be uniformly spread to the edges of the cloth, otherwise the heat will dry it, and cause pain and difficulty in detaching it from the surface of the burn when it is removed. This poultice gives immediate ease, assuages the sense of heat, resolves or moderates the inflammation, promotes suppuration and sloughing, and forms a soft nidus in which the granulations may repose with safety and comfort. This poultice may be continued until suppuration is fairly established, and if that is not too abundant, even until the granulations show a disposition to exuberance.

The Elm-bark poultice is a very good one, and although it approaches very near the former in its effects, yet I do not think it equal to it. When used, the bark should be reduced to a perfect pulp, if possible, by pounding it, having previously softened it with hot water. It is then spread as the former, applied in the same way, and may be continued as long without removal.

Poultices made of other substances have been highly recommended. They are such as thickened milk, bread and milk moistened with vegeto-mineral water, and scraped potatoes &c. These poultices resemble the former, but no two can be said to be precisely alike, and therefore selections must be made from among them according to the judgment of the surgeon.

Another set of substances of which dressings are made, are distinguished by possessing an oily quality. A dressing of this kind, which I have frequently made to burns of the first class, is thus applied. Take linseed oil and carded cotton, annoint the surface of the burn well with the oil, and then spread the cotton pretty thick and uniformly over the whole. This has been esteemed a successful method of dressing burns, and I agree that it is so. But there are objections to it. It is a very severe application for a time. The oil soon dries on the surface of the burn, and the cotton is apt to stick; and besides, it is a difficult matter to keep the dressings on as first applied.

The dressing requires renewal at short intervals to keep the parts comfortable.

Another oily substance which suits these burns, is hogs lard. The best plan of applying it, (if the part will admit) is to bury the part in the lard; as soon as this becomes thin, let fresh lard be applied, and let this process be continued until the pain and heat of the burn have ceased. After this period any other suitable dressing may be resorted to.

As we have before remarked, burns of the first class are apt to be followed in a short period of time with a constitutional disturbance, amounting to either inflammatory or irritative fever. The phenomena thence resulting, both local and general, have been detailed in a former part of this essay. To oppose this febrile state, the general remedies must be resorted to. These remedies are such as blood-letting, cooling purgatives, diaphoretic refrigerants, diluents, and a cool regimen. These remedies must be pushed to an extent suited to the grade of excitement, the extent of injury, age of patient, season of the year &c. If the excitement is highly inflammatory, the burn severe and extensive, the patient young and robust, and the season warm, then all the remedies may be urged to an extent that would not be warranted in opposite circumstances. If the fever is irritative, and pain is distressing, opium may be prescribed in conjunction with a judicious antiphlogistic treatment.

The stimulant treatment now claims our consideration. It is suited to the second class of burns, and in cases of this class we have before remarked, that the vital powers, resident in the part injured and the general system, are greatly exhausted. Were this not the case, stimulants could not be admissible in burns at all. But such being the fact, they come in upon the principle by which one stimulant is made to substitute another, when the powers of life are nearly prostrated by the first used. In this course of substitution, it does not appear that the last stimulant urges the excitement much above the healthy standard, while it favours its return constantly to that point by artificially maintaining it near that point. All this is exemplified, in the good effects of opium and other stimulants in that low

state to which the system is sometimes reduced by a free and long continued use of ardent spirits.

Like the antiphlogistic, the stimulant remedies are local and constitutional. The local are such as hot spirits, oil of turpentine, spirits of camphor, alcohol &c. Some surgeons prefer incorporating the turpentine with other substances, forming salves. We may choose our own methods of making the application, but care should be observed that they are kept from the sound skin, and that they are laid aside so soon as reaction begins to manifest itself in the part. It would be well perhaps in all cases to reduce the strength of the article used gradually.

Those whose action is directed upon the system at large, are such as æther, wine, brandy, opium &c. The doses of these articles, and the length of time they should be continued, are circumstances to be determined by the urgency of the case, and the protraction of the state which warrants thir exhibition.

ART. V.—*Thoughts on the Moulting of Birds. By* CHARLES CALDWELL M. D. &c. &c.

ON an attentive consideration of the ever changing aspects which Nature presents to us, in her several departments, we cannot escape the conviction, that she delights in variety and beauty, no less than in usefulness. No sooner has one course of things, or one condition of objects, continued so long, as to become tiresome and unseemly, either from sameness and monotony, or a deterioration of appearance, than it undergoes, by established laws, such a change, as gives to it again a power to please.

This is, at once, illustrated and proved, by the mutations sustained by the vegetable and animal kingdoms, corresponding

to the varying seasons of the year. If indefinitely protracted, even in unfading beauty and loveliness, the blossoms, and fragrance, and sprightliness of spring would cease to delight us. So would the luxuriant and glossy foliage of summer, in common with the fruits of autumn, although delicious to the taste, and richly decorated in gold and purple. It is not, therefore, without feelings of relief from the past, and a sentiment of welcome, as respects the future, that we contemplate the approach even of winter,

"Sullen and sad, with all his rising train;
"Vapours, and clouds, and storms—"

But these changes from the aspect of one season to that of another gratify us the more, and are the more requisite, inasmuch as the charms of spring, the splendors of summer, and the rich and mellowed beauties of autumn fade of necessity, and lose their attractiveness, in conformity to the irrevocable laws of creation. Nor from these laws is the sternness of winter exempt, but submits to their control, in common with the other seasons of the year.

In the animal and vegetable kingdoms many of these mutations occur, in conformity to the same ordinances of nature. They obey the same principles, when the vegetables annually lay down their leaves, and the animals their hair, feathers, and other external ornaments, or weapons of annoyance and defence. Nor can the falling off of the human hair, from sickness, age, or intense cold, be traced to the influence of any other principle.

These remarks are meant to be introductory to a few observations, which it is our intention to offer, on a remarkable change, which occurs periodically in a certain department of the animal creation, and one of whose objects appears to be, the renovation of faded beauty.

In the Transactions of the American Philosophical Society, Vol. III. Part II p. 292, new series, is published an interesting paper on the "Moulting of Birds" by George Ord Esq. of Philadelphia. The avowed object of the writer is to prove, in opposition to the opinion of certain other naturalists, that

this process occurs but once a year. In the establishment of the *fact* we think him successful, but doubt the soundness of his philosophy in relation to it. The following are his words on this topic.

"The intention of nature, in renewing the covering of birds, appears to be *a reinvigoration of those powers which are necessary to the propagation and consummation of the animal.* After the breeding season is past, the period of moulting commences. The effects of this exhausting process are well known. When the bird recovers its strength, we find it in a new garb, which advances to perfection in proportion to its necessities: those which migrate to great distances standing in need of a speedy maturation; whilst others continue in the act of moulting between three and four months" pp. 293—4.

It would be exceedingly difficult, we think, to show, in what way either a mere new covering of feathers, or the process of producing it reinvigorates a bird. Nor do we deem it less difficult to render it even probable that it does so.

As corresponding better with other phenomena, it appears to us much more likely, that the growth of new feathers is the result of a previous "reinvigoration" arising from other causes. A suitable covering of feathers tends, no doubt, to preserve the health and vigour of birds, by protecting them from the changes and inclemencies of the weather, enabling them to take the requisite amount of exercise, by flying, and guarding their skin against the assaults of insects, and other external injuries. But here, we apprehend, its strength-bestowing benefactions end. It aids, then, in sustaining strength; but there is no ground to believe that it is instrumental in imparting it.

Nor are we inclined to call moulting an "*exhausting process*" as if it were the cause of the feeble condition of the bird at the time. We believe it to be rather the result of exhaustion and debility, induced by other preceding causes, which can be easily recited.

"*After the breeding season is past,*" says Mr. Ord, "the period of moulting commences." This is true; and the enfeebled condition of the bird, during the "period of moulting" is the

natural consequence of what it has done and suffered during the "breeding season." By contemplating, for a moment, what it has thus done and suffered, we can scarcely fail to be convinced, that exhaustion and debility must be necessarily the issue of it.

In all animals that experience, during the year, but one period of *love*, that passion, when it occurs, is extremely ardent, and its gratification fierce, tumultuous, and exhausting. Hence the truth of the maxim, "*omne animal post coitum triste.*" And hence all such animals, after the season of their amours, are in a debilitated condition. Of the truth of this the elk, the mouse, the common deer, and the buffalo furnish well known instances.

This is still more peculiarly the case with birds, whose temperament is warmer, their degree of vitality higher, and their affections stronger, than those of quadrupeds. Their temperature being several degrees above that of man and other animals, testifies to the superior ardour of their constitution.

The violent and extensive, perhaps I might have said excessive, indulgence of their loves alone, then, acts on the feathered race, as a debilitating process. Nor is this all. The work of nidification, in which they now engage, is exceedingly laborious and exhausting; the more so, as they pursue it so industriously, and are so perfectly absorbed in it, as not even to provide for themselves their usual supply of food.

Their nests being constructed, and their eggs deposited, next comes the tedious business of incubation, which requires, in some way, the united and uninterrupted toil of both the male and the female. And, during this period, the supply of food, again, appears to be scanty; less abundant, certainly, than in times of leisure.

Last of all, but by no means the least toilsome and debilitating, come the feeding, attending, and rearing of the young. On the contrary, this is by far the most flesh and health and life-consuming process of the whole. Added to incessant labour and a defective supply of food, it is attended with a degree of solicitude and care, that is wearisome and wasting, perhaps beyond

all the other causes that have been mentioned. On this topic we shall only further observe, that this scene of love, labour, privation, and anxiety lasts always many weeks, and, in most instances, several months. No wonder, then, that the actors in it should be worn down, feeble, and injured in their health. Let man himself thus labour, fast, and suffer anxiety, his strength will be shattered, and his health impaired.

Such precisely is the condition of birds, as their appearance, and all their movements and manifestations indicate, by the time their young are sufficiently matured to take care of themselves. The brilliancy of their colours is faded, their sprightly and graceful playfulness and cheerful notes no longer delight us. Their feathers have lost their glossiness, and look old and tattered, and, instead of their late activity and flying abroad, they seem now to delight in concealment and rest. They manifest the feebleness and even drooping sadness, which result from exhaustion and shattered health.

Nor are the causes of this state of things concealed from us. When, by labour and scanty sustenance, the vitality and strength of an animal are diminished, it suffers most in its extremities and circumference. In other words, its *skin* is most seriously affected, as being most remote from the sources of nutriment and life. But we need not add, that it is by this tissue, that the feathers of birds are produced, nourished, and supplied with the secreted matters which impart to them, at once, their colour and their gloss.

The skin of these animals, then, being deprived of its nourishment, is wanting in its usual and requisite quantity of blood. Of this state of things the effect is obvious. It withholds, of necessity, from the feathers, the matter of both sustenance and colour. Hence those articles of ornament, action, and defence are literally starved. They, therefore, loose their beauty, wither, die, and escape from their sockets. Their fastenings, by which they had been secured in their positions, are dissevered, by the process of absorption; and the animal, moved by instinct, assists in the shedding of them, by the flapping of its wings, the frequent shaking of itself generally, and the employment of its beak.

Thus is the covering of the bird cast off. But the process of moulting is twofold, the removal of old feathers and the reproduction of new ones. And the latter part of the process, being the reverse of the former, arises from an opposite state of things. It is the natural issue of the reinvigoration of the animal's system.

The bird exempt now from both labour and care, and still able to procure for itself a sufficiency of food, begins to thrive. And, as its fulness, bulk, and vigour increase, its new feathers grow, until, by the time its strength of muscle is such as to justify it in commencing its journey of migration, its plumage generally is competent, as a covering, and its wing and tail-feathers as means to sustain and direct it, in its passage through the air. But the feathers have not yet attained their perfect colouring, because they are immature. They are even younger than the feathers of the recent brood. Hence the difficulty of distinguishing the male from the female, and the young birds from the old, after the latter have completed their moulting. In the United States, and in other corresponding latitudes, this is the state of things in September and October. Nor, through the winter, does any very material alteration occur. The season is too bleak and ungenial to mature and beautify the plumage, or restore to the birds their vivacity and song.

But a few weeks previously to the next period of their loves, things begin to change. The sun is returning from his sojourn beyond the equator, and awakening nature once more feels and acknowledges his quickening influence. All that has life not only seems to hail his approach, with expressions of joy, but enriches and beautifies its attire to receive him. No wonder that the ancients regarded and worshipped him as the source of song, and every other manifestation of genius and taste.

It is now again that the feathered race begin to manifest their entire vigour, action, and sprightliness. Now that their melody resounds through the groves and forests, their plumage assumes its finest gloss and richest colouring, and that they delight in frequent and frolicksome exhibitions of their activity and strength. Were we to allege that the rare elegance of this wedding garniture, united to the sprightly and attractive move-

ments, and the thrilling song, of the birds that wear it, is intended to heighten and actually does heighten, the mutual attachment and passion of the sexes, on the ground of the loveliness of beauty, grace, and melody, the conception would not be more unreasonable than many that are recorded in works, not of fancy and fiction, but of sober science. That this is the case, we neither positively assert, nor deny. We only add, that sundry manifestations seem to render it probable. For man to imagine, that all that is beautiful, graceful, and melodious in the feathered race is intended for his exclusive gratification, is to assume to himself fully as much as his station justifies, and much more than he is able to prove. There seems little doubt that the birds themselves, as well as he, are alive to their own beauty and their own melody. The season of love once more begins, to be succeeded by the toils, privations, and cares of nidification, incubation, and the rearing of young, which produce again the exhaustion and debility, with all their concomitants, already recited.

Resolved to make every thing bow to hypothesis, some individuals contend, that the rich and variegated colouring assumed by the plumage of birds, in the spring, is attributable to the chemical action of the increasing solar light. This is as palpable an error as man can imagine. The light of the July, August, and September sun is more intense than that of the sun of March or even April. But the former could not produce the brilliant hues to which we have referred; because, as already mentioned, the colours of the summer and autumnal plumage of birds are faded and faint. If then, the stronger light could not chemically die the hues in question, *a fortiori*, the weaker is incompetent.

Chemistry has no concern in the process. The plumage of birds receives, in the spring, its gloss and colours from the process of secretion, which derives its existence exclusively from the influence of vital laws. By means of their stimulating and salutary impression on the skin of birds, the light and heat, more especially perhaps the latter, of the vernal sun draw into that tissue a more copious flow of blood, and excite in it a higher

degree of action. The consequence is, a richer and more abundant coloured secretion, which is diffused through the feathers, and gives to them their tints. Could any other substance stimulate the skin and the system in exactly the same way, the result would be the same. Colours precisely similar would be secreted. On this topic generally, the following fact corroborates our views.

On the occurrence of a bleak and disagreeable morning, a humming bird was caught benumbed by the cold. It was, at first, believed to be dead. Its plumage, especially on its throat and breast, was of a pale and sickly colour. Symptoms of animation being, at length, perceived in it, it was gradually and cautiously introduced into air of a higher temperature. It was cherished, in a particular manner, by the warmth of the hand. Its torpor disappeared, by degrees, and, in a few hours, its entire vigour was restored. So were the beautiful tints of its plumage. Instead of the paleness, which had lately overspread them, its throat and breast were decorated with scarlet and green. In proportion as its vigour and activity returned, so did its colour.

Of this fact the explanation is obvious. During the torpor of the bird, the cutaneous secretion was entirely suspended. Hence the plumage had lost its brilliancy. But no sooner was this secretion restored, than it passed into the faded feathers, and renovated their colouring. Nor, as the bird was under cover, did the sun now shine on it at all. The warmth communicated to it came from other sources. It is for the same reason, that during a warm and sunshiny day, the delicate colours of every small bird, are more bright and beautiful, than they are when the weather is bleak and chilling.

We have asserted that the birds change their feathers in conformity to a general law of nature; and that this law is identical with that, in obedience to which, quadrupeds change their hair, snakes their skin in the spring, perennial vegetables their leaves, and man himself the natural covering of his head. The following facts sustain this position.

A man, in the vigour of life, experiences a severe attack of typhus fever. By weeks of confinement and suffering he is

greatly reduced and enfeebled, but ultimately recovers. What is the consequence? An entire loss of his hair. Why? On account of the exhausted condition of his skin. The richness of the soil is, for the time, worn out, and therefore does its production wither, die, and fall. Nor does a new crop put forth, until, by the operations of health, the system generally, and with it the skin, is reinvigorated and replenished. Here the system is not, at first, debilitated, because the hair comes out; the hair comes out, because the skin is exhausted. Nor, in the second place, is the system reinvigorated, because the hair is reproduced; the hair is reproduced, because the vigour and secretions of the skin are restored.

A young man, born and raised in the south, emigrates far to the north, where the cold is intense. Let it be to St. Petersburgh or Archangel, in Russia. Unless, during the first winters of his residence in his new place of abode, he is extremely careful to protect his head, and even his whole person, by a covering of fur, he loses his hair, in consequence of the withering action of cold on his skin. Nor will his hair ever be fully restored, unless he return to a warmer climate.

In consequence of the fierce and wasting indulgence of his loves in the autumn, a deficiency of food during the winter, and the deleterious action of cold on his skin, the stag, as already intimated, loses, toward spring, both his hair and his horns. But, from a reinvigoration of his system, by a freedom from passion and fatigue, and an abundance of succulent and nourishing food, they are both restored, in the course of the summer; and, by the coming of autumn, he is again in a condition to contend for his female, and propagate his race. It may not be uninteresting to observe, that it is not the covering of animals alone that suffers a mutation, after the exhaustion of the season of passion. Their genital organs change and dwindle to such an extent, that it is not easy to distinguish, by that mark, the male from the female. This is the case with birds, especially small ones; and, indeed, more or less, with all animals, whose sexes unite but once in the year. Even in the mole tribe, whose genital organs

are unusually large during the season of their loves, at other times they can scarcely be found.

Why, toward the close of the summer, does the foliage of the forest begin to fade? and why, during the autumn, do the trees drop their leaves, and allow them to be scattered by the wind? The answer is easy. By the process of vegetation generally, throughout the spring and summer, including actual growth, as well as the production of flowers and fruit, the trees are exhausted of their vitality and vigour, and are able no longer to nourish their leaves. And the hotter and drier, and therefore, the more fatiguing and wasting in its influence the summer is, the earlier does the fading of the foliage commence. Nor can the growth of the forest put forth again, until reinvigorated by the repose of the winter. And the more profound that repose is, from the severity of the winter, the more rapid is vegetation, on the return of spring. Hence the suddenness, with which, at the close of a Russian or Siberian winter, the plains and forests are covered with green.

Such appears to us to be the philosophy of the moulting of birds, and other analogous physical processes.

Mr. Ord, we repeat, is correct in his opinion, that birds moult but once in the year. Observation confirms this, and changes it from opinion to fact. Principles speak the same language. Birds, in this climate, breed but once a year. They undergo, therefore, but once, the debilitating process, which leads to moulting. Did they breed twice a year, they would moult as often. But in that case, their lives would be shorter than they are, unless their constitutions were altered to suit their economy.

Might we venture to offer a suggestion, participating of advice, to a naturalist so well informed and respectable as Mr. Ord, we would say, that were he to devote himself, for a time, to the study of anatomy and physiology, both morbid and healthy, the knowledge thence derived would aid him very materially, in his favourite pursuit.

ART. VI.—*A Dissertation on the non-specific Nature, and non-mercurial Treatment of Venereal Affections.* *By* CHARLES SHAW, M. D. *of Jefferson county, Mississippi.*

[NOTE.—The opinions contained in the following pages, and most of the arguments brought forward in support of them, being mine only by adoption, I will, once for all, state the sources from which I have derived them. The following are the principal: The treatises of Hunter, Swediaur, and Carmichael, on the Venereal; a paper by Dr. Ferguson on the venereal disease in Spain and Portugal, contained in the 4th vol. of the Medico-Chirurgical Transactions, and two papers in the 8th vol. of the same work, on the treatment of syphilis without mercury, one by Mr. Rose, the other by Mr. Guthrie; Hennen's Military Surgery; and lastly, the work of a late French author, M. Des Brus, a review of which is contained in the 1st and 2d vols. of the American Journal of the Medical Sciences.]

FEW of the opinions entertained by the physicians of the latter part of the fifteenth, and commencement of the sixteenth centuries, respecting the nature and treatment of diseases, have been adopted by their successors; but, as if to compensate in some degree for this general neglect, the most implicit faith has hitherto been accorded to one, in the establishment of which they were more immediately concerned. Syphilis, by general consent, but with little apparent justice, dates the commencement of its history at the period above mentioned, and from that time until the present moment, it has been regarded as a specific disease, the product of some peculiar poison of unknown origin, specific in *its* character, and capable of generating an offspring similar to itself. Its treatment, too, has been considered as established on a basis too firm to be shaken; the experience of more than three centuries having fixed on mercury as the *specific* and only remedy.

Different opinions, however, in regard both to the nature and treatment of Syphilis, have been, of late, promulgated, and by men whose opportunities for observation on this subject have not been inferior to any, and whose ability to improve these opportunities none can doubt. They have ventured to assert, that it is not a specific disease, nor the product of a specific poison; and that it can be safely and effectually cured by other means than the employment of mercury.

Of opinions so directly opposite, one must necessarily be false; it therefore becomes an object of solicitude to ascertain which is correct. This I have endeavoured to do; and as the one which I have settled on as such, is the one which is esteemed otherwise by a large majority of those who have any opinions at all on the subject, I hope a desire to justify my choice will be considered a sufficient apology for bringing forward those facts which tend to support it, though opposed to the views of the teachers in this institution.* As a further apology for the inquiry into the possibility of curing Syphilis without mercury, I might urge the very interesting fact, that there exists a very considerable class of persons, who, from a scrofulous diathesis, or peculiarity of constitution, are unable to use this remedy in any form, without inducing effects as terrible as those it was intended to remove; and who, consequently, if the disease in question was curable by no other means, would be exposed to its progressive ravages, without a hope or possibility of relief.

I have also been induced to question the truth of the opinion which gives to mercury a *specific* control over Syphilis, from a belief I am under, that there does not, and, from the very constitution of our nature, there cannot, exist such a thing as a specific remedy for any disease; the ever-varying condition of the system rendering it impossible that any agent whatever should have the power, at all times, of correcting the derangements produced by any specified cause. Moreover, a belief in the existence of specific remedies is calculated, by drawing our atten-

*This paper was submitted by the writer, to the examination of the President, Trustees, and Medical Professors of Transylvania University, as an Inaugural Thesis, for the degree of M. D. on the 20th of February, 1829.

tion to a vain search after them, to paralyze our enquiries into those general principles which govern the healthy and morbid actions of the system, and which regulate the operation of remedial agents. I am anxious, therefore, to free myself from it; and I indulge the hope that the period is not far distant, when it will have as few votaries as, in my opinion, it deserves to have.

Of the existence of specific diseases, in the present state of our knowledge, there can be no question; and probably there never will be. Assuming one, therefore, the small-pox, admitted by all to be such, as a standard of comparison, I proceed to examine the claims of Syphilis to be ranked in this class. A clear idea of what is meant by *specific disease*, must necessarily be kept in view. Such a disease is understood to be the product of a specific poison; which poison has the power at all times, and in every variety of circumstances, of producing a disease always so precisely the same in its course and symptoms, as not to be mistaken for any other, and which by its own peculiar action produces a poison possessed of the same powers as that by which it was itself produced. The small-pox, our standard of comparison, will illustrate this definition, and the following cases will show that it will not apply to Syphilis. They are taken from the first volume of the American Journal of the Medical Sciences. "Six friends, after a hearty repast, had connexion with the same girl, who communicated the pox to them all. The disease, however, manifested itself among them with *very different* symptoms. Two of them had chancres and buboes; two others gonorrhœa; the fifth had a chancre; and the sixth a single bubo. One of those who had chancres and buboes was soon cured, by a regular treatment with frictions. The other with the same affections, had his bubo to suppurate, the skin to separate, become spongy and perforated. He became dejected, salivation took place, the submaxillary glands became engorged, the discharge from the chancre ichorous, he lost his strength and sunk. He who had only a chancre, burnt it out, and experienced no inconvenience after marriage, his children proving healthy. Of the two who had gonorrhœa, one experienced it in its mildest form, whilst in the other, on the contrary, it was attended with

exceeding virulence, and resisted the efforts made to cure it for ten months." The variety of diseased appearances presented in the foregoing extract, arising from the same cause, are totally irreconcileable with the belief, that that cause was of a specific nature, and capable, like the small-pox, of establishing its own peculiar train of actions, independently of the states of the systems of those on whom it operated. An explanation can only be found in the different degrees of excitability in the different persons; and the effects are such as might be expected from the application to the parts, of any active irritant not specific in its nature. Many cases of a similar import are to be found in the same author from whom the foregoing are taken; but it is unnecessary to cite them. The inference from them is easy to be made, and conclusive in its character. It is only necessary to bear in mind, what would be the effect of exposing a number of persons, susceptible of taking that disease, to the contagion of small pox.

The definition of a specific disease, further requires, that it shall be so characteristic in its symptoms and appearances, as always to be known, and never to be mistaken for, or confounded with, any other affection; and here again it does not apply to Syphilis. No person of judgment will now venture to assert, when an ulcer on the parts of generation is presented to him, whether it is syphilitic or not. The most experienced confess their inability to do so. And the fact is conclusive, that there exists no characteristic mark of this disease. Mr. Abernethy tells us, he never knew when he was treating a case, whether it was Syphilis or not, until he had cured it; and Mr. Carmichael found such variety in the appearances presented by venereal sores, that to reconcile them with the idea of the existence of a specific virus, he was forced to the conclusion that there exist several distinct species of it, each capable of producing its own peculiar train of symptoms. The following extract from another late writer will still further illustrate the obscurity attending the diagnostic marks of Syphilis. "In some cases, (says he) I encountered all the marks of syphilitic ulcers strongly marked and determined; I admired and pointed them out to the

students present at my visit, and have been told the next day, that they were produced and kept up by the diurnal application of caustic."* This is the testimony of a man who had the venereal department of a military hospital under his charge, and who, on other subjects, has manifested sufficient powers of discrimination. Now, can these various appearances be attributed to a *specific cause*, capable under all circumstances of producing its own train of phenomena? Who has ever found any difficulty in pronouncing on small-pox, or who has ever mistaken it for any other disease? The doctrine of *specific cause* and *specific effect*, so much insisted on by a celebrated lecturer, will have to undergo some important modifications before these discordant appearances can be made compatible with a belief in the specific nature of the cause of Syphilis.

We thus see, that Syphilis, in no respect, conforms to the admitted definition of a specific disease. But there are other considerations which render the justice of its claim to this title still more problematical. Of this kind is the well known fact of a female infecting those who have connexion with her, and even continuing to do so for years, without giving any manifestations of disease in her own person. A very fine instance of this kind is given by Dr. Ferguson in his paper on the venereal disease, as it appeared in Spain and Portugal. An opera dancer in the city of Lisbon infected half the officers of the British army whilst they were quartered near that place, some of whom suffered the "most melancholy of all mutilations," and yet the lady continued in the uninterrupted discharge of her various employments during their stay, without any inconvenience whatever. Now we cannot for a moment believe, that a specific poison existed in this case, as the cause of the diseases produced; for we cannot imagine such a poison to exist in any person in a state sufficiently active to produce disease when applied to others, yet produce no manifest effects on the person in whom it was lodged. An attempt will be made in a subsequent part of this paper, to show how it is that disease is produced in such cases

*Des Brus, as quoted in the Amer. Journal of the Med. Sciences, vol. I, p. 378.

as the foregoing, and the explanation will afford us an additional apology for the disbelief in the specific nature of the cause.

The frequent appearance of spontaneous cases of Syphilis, or those arising where there has been no exposure to a foreign cause, by showing that such a cause is unnecessary to account for their production, tends still further and more strongly to disprove its existence. That such cases do frequently occur, is admitted, I believe, by all the writers on this subject; and a very striking one, in particular, is given by Dr. Emerson, the American editor of Mr. Carmichael's work on venereal affections. Such cases also furnish us material assistance in arriving at the true nature of the cause of Syphilis., and further use will be made of them hereafter.

Syphilis has been produced in a healthy person by inoculation with the pus from a venereal ulcer; and this has been brought forward as a proof incontestible of the existence of a virus; but I am disposed to think, that an unbiassed person, after a careful examination of these experiments, will feel himself constrained to draw an opposite conclusion. These experiments rarely succeeded in producing any thing more than a mere local irritation, when the pus was inserted into any other tissues of the body, than those constituting the usual seat of the disease in question. Now it is a matter of no consequence into what part of the body you insert the matter of small-pox, the disease is alike produced wherever the virus is inserted, and if there existed, as the cause of Syphilis, a specific poison capable of establishing, under all circumstances, its own peculiar train of symptoms, analogous effects should follow inoculation with it as with that of small-pox. The symptoms produced by inserting the venereal pus into the organs of generation are plainly owing to the irritation produced, in those highly vascular and sensible parts; independently of any specific action, as it will be shown hereafter, that all these symptoms can be produced by the application to these parts of any highly irritating substance. Inflammation of the glands of the groin, causing buboes, has been thought to afford evidence of the existence and absorption of a virus; but it is difficult to conceive why, since the same

effects are produced by numerous other causes not deemed specific, and where no absorption could have taken place, as from a thorn or rusty nail sticking in the foot; and in the plague, and other malignant forms of fever. It is indeed strange that the absorption of virus should ever have been thought necessary to account for the constitutional effects of Syphilis, when it is known that inflammation of the absorbents, followed by suppuration of the glands, derangement of the constitution, and death, have been produced by the prick of a clean needle through a leather glove; and still more strange, why this virus should be required to possess a specific nature, since the same effects follow the irritation produced by any of the various parts of the body in a state of decomposition; a fact which dissectors have frequently experienced, and of which the late work of Mr. Travers on constitutional irritation furnishes numerous examples.

The foregoing constitute the principal objections to the specific nature of Syphilis, and the cause producing it; and I cannot but think them sufficiently conclusive. Taking it for granted, then, that there does not exist any specific poison, as the cause of Syphilis, the question, what *is* the cause? very naturally presents itself. If it can be shown, that all the symptoms, not only of Syphilis, but of all the various grades of venereal affections, can be produced by ordinary, *unspecific* irritation, and if an irritant possessed of sufficient power can be shown to be always present in the circumstances in which they are usually produced, I think the cause will be sufficiently manifest; and we shall be able to substitute for the vague notion of a specific virus, something of a more tangible and comprehensible nature.

The following facts will show, that all the symptoms of the different forms of venereal affections can be produced by ordinary irritation. Dr. Swediaur, as he informs us in his work on Syphilis, by injecting a strong solution of sal-ammoniac a short way up his urethra, produced a most virulent gonorrhœa, or at least all the symptoms of that disease, such as a profuse discharge, ardor urinæ, nocturnal erections, &c., requiring six weeks to effect a cure. And Dr. Emerson, the editor of Mr.

Carmichael's work, has seen the application of a caustic to the prepuce, for the removal of an indolent wart, produce a sore not to be distinguished from the true chancre, as described by Hunter, and which was followed by all the constitutional manifestations of Syphilis in a most violent and aggravated form. Now I know it is objected, that such cases are not cases of *true* gonorrhœa and syphilis; but nothing can be more idle and futile than such an objection, for we know no means of judging of the nature of a disease but by its symptoms and effects; and, judging by these, no person could have distinguished either of the foregoing cases from those supposed to be produced by the imaginary virus. Common sense, therefore, declares them to be of the same nature, and they prove what they are brought forward to prove, that any sufficiently intense irritation to these excitable parts *can* and *does* produce the affections in question.

Now for the application of the point established by the facts just given. It is well known that the secretions of any part of the body, when from any cause they become deranged or vitiated, acquire highly acrimonious and irritating qualities, and will produce diseased action when applied to similar parts in a healthy person. It is a common occurrence for a disease resembling gonorrhœa so exactly, as not to be distinguished from it either by its symptoms or progress, and therefore, according to my logic, a true gonorrhœa, to be produced by connexion with a female labouring under leucorrhœa. An amusing instance of this kind is to be found in one of the volumes of Mr. Abernethy, of a physician who "had to do" with one of his female patients in this condition, and who was punished for his incontinence with a 'most virulent clap.' Dr. Chapman, also, gives the case of a person whom he had frequently under his charge, for affections arising from leucorrhœa in his wife, varying in their appearance from that of a mild gonorrhœa to the true chancre, and which finally destroyed nearly the whole of the glans penis. Many cases of a similar kind might be brought forward, but it is unnecessary, as the frequency of their occurrence is admitted by all. Analogous effects are likewise found to be produced by the depraved secretions from other parts of the body. We

know, for instance, that the secretions from the eyes of a person affected with ophthalmia will produce disease when applied to the healthy eyes of another person; and it is said the discharge attending a *coryza* will produce a similar result. Now when we see these secretions by a slight departure from the healthy state, enabled to produce such effects as the foregoing, may we not infer that a still greater departure from this state will enable them to produce effects still more marked—even the appearances constituting the disease termed Syphilis? Such an inference appears to me not only an allowable and fair one, but to be irresistibly forced on us by the facts; and even if it were a mere hypothesis, unsupported by any facts whatever, still there would be strong presumptive evidence of its truth, from the facility it affords us in accounting for many of the phenomena attending on venereal affections, wholly inexplicable on any other grounds.

By viewing the subject in this light, we can find a satisfactory solution for the fact formerly mentioned, of a female infecting others without giving any manifestations of disease in her own person; it being a well known fact that the secretions of any part, however vitiated they may be, have not the power of irritating the surface which pours them out, it having become habituated to that impression in their gradual and imperceptible change from a healthy to a depraved state. But when applied, in this highly vitiated condition, to similar organs in a state of health, and consequently unaccustomed to their impression, the irritation they produce is so great that disease is the consequence. Thus, the secretions from the eyes of a person with ophthalmia are quite unirritating to the surface which pours them out, but when this secretion comes into contact with eyes in a healthy condition, disease is produced; and the thing is precisely the same with the organs of generation, as appeared in the case of the opera dancer already given. Various causes, such as neglect of cleanliness, excessive stimulation, &c. may change the healthy nature of their secretions; but the change is effected so gradually, that no evil consequences are felt. When applied, however, after they have arrived at a certain state of vitiation, to the healthy organs of another person, irritation and

inflammation ensue, with all the various symptoms of venereal affections.

This view of the subject also explains to us the endless variety in the appearances presented by different cases of syphilis, and the consequent variety in the descriptions given by the authors who have written on it. In proportion to their departure from a healthy condition will be the intensity of the irritation produced by the vitiated secretions; and in proportion to the intensity of this irritation will be the severity of the symptoms in the disease produced. It follows of course, therefore, that there must exist a variety in the appearances of the diseases thus produced, wholly incompatible with any specific cause. Now it is admitted by all, that this variety exists; it has been shown, moreover, that any intense irritation applied to the organs of generation will produce symptoms of syphilis; and it has also been shown, that a depraved state of the secretions of these parts, from whatever cause induced, has the power of producing this irritation. From the whole then, I draw this plain conclusion, that as these depraved secretions are always present in the circumstances in which the disease is produced, they are its cause, and to them alone it can be ascribed.

So much for the *cause* of syphilis. Its treatment might now with propriety be given, but the details of this do not come within the design of this paper: I only propose to show, in connexion with its *non-specific* nature, the incorrectness of the opinion which denies the possibility of curing it by other means than the employment of mercury.

If the foregoing views of the nature of the disease be correct, there can exist no doubt of the possibility of effecting a cure by different plans of treatment; but laying aside all conclusions drawn from the nature of the disease, I shall rely for support solely on facts established by the most ample experience. Before entering on this subject, however, I wish it to be distinctly understood that I am not objecting to the use of mercury in the treatment of venereal affections; on the contrary, I believe, when administered in a proper manner, it is the remedy best suited to a vast majority of the cases that occur; not, however, because

of any *specific* controul it possesses over them; but, because in these, as in most other diseases, it is the most powerful agent we possess in breaking up morbid actions, and establishing those of a healthy character. All that I wish to establish is the *possibility* of effecting a safe and speedy cure without it; so that in the event of a case occurring, in which, from peculiar circumstances, it could not be used, I should feel safety and confidence in resorting to the employment of other means.

Taking it for granted, that mercury was absolutely necessary for the cure of Syphilis, Mr. Hunter has adopted this as a test of the disease, and considered all those cases which admitted of a cure without this remedy as not syphilitic, however much they may have resembled his genuine cases in symptoms and appearances. This, however, is only one of the many errors which his authority rendered current for a time, and which have been corrected by later and more unbiassed observers. Rejecting this test, therefore, which, if true, would render the present enquiry idle and unnecessary, I proceed to state the facts which prove the possibility of curing every grade of venereal affections without any recourse whatever to the use of mercury.

In Spain and Portugal, where venereal affections are as common perhaps, as in other countries, mercury is rarely used in any stage of these affections; yet cures are effected without any of the evil consequences ensuing which we are taught to believe would follow its omission. Dr. Ferguson, who was at the head of the medical department of the British army in those countries, for several years previously to his publication on this subject in 1812, gives us the most explicit testimony on this point. He tells us that the native practitioners *never* used any other than mild vegetable remedies in the early stages of Syphilis, and that cures were effected by them as speedily, and indeed much more speedily, than could be done among the British soldiers by the most active exhibition of the *specific*. The fact of cures being effected in these countries without mercury, is admitted by all parties; but its force is attempted to be evaded by *supposing* that, owing to the general diffusion of Syphilis among them, the Spanish and Portuguese constitutions have be-

come so habituated to its impression, as to be but slightly affected by it, and therefore throw it off with little or no assistance; while the necessity of resorting to mercury in the treatment of foreigners who contract the disease there, is indicated by the violence and rapidity with which it runs its course in them. This supposition appears to have been acted on by the British army surgeons; but the result of their practice was not very flattering, as we are told by Dr. Ferguson that more instances of the "most melancholy of all mutilations" happened from Syphilis among the British soldiers during the four years they were in that country, than the records of all the hospitals in England could furnish for the last century. Nor is it difficult to account for this, when we recollect the well known fact, that persons going from a northern to a southern climate, have their susceptibilities to inflammatory affections greatly increased, and that these affections run their course with extraordinary violence and rapidity. If this fact had been kept in view by the British surgeons, and a rational practice adopted, these 'mutilations' need never have happened, as is finely exemplified in a case related by Dr. Ferguson. An officer, a friend of his, had contracted Syphilis; but when he applied for relief, the inflammatory symptoms ran so high as to threaten the loss of the parts, before the *specific action* of mercury could be induced. To save them, therefore, he was bled, puked, and purged, most promptly and copiously, and thereby in a short time restored to perfect health. This case shows most clearly, that foreigners contracting the disease there, require no peculiarity of treatment, save what was rendered necessary by the inflammatory diathesis induced from change of climate. But admitting that the Spanish and Portuguese constitutions have become insusceptible to the ravages of Syphilis, and consequently that no inference, applicable to the treatment proper in other countries, can be drawn from that which proves successful in these; still we have the most abundant testimony to prove the possibility of effecting cures without mercury, when no such fortunate peculiarity of constitution prevails.

In France, where the principles of physiology are at present

made the basis of practice to a greater extent than, perhaps, in any other country, the notion of the specific nature of venereal affections has been discarded by several eminent physicians; and a practice adopted in conformity to the opinion that they are nothing more than the product of ordinary irritation, modified by the peculiar structure and functions of the parts. The results obtained by these gentlemen are entirely conclusive, as to the possibility of safely dispensing with mercury; and they even go far to show, that more speedy, safe, and effectual cures are to be thus obtained, than by the mercurial practice. It will be sufficient to state the facts furnished by one of these gentlemen. M. Des Brus, the surgeon in chief to the venereal department of a large military hospital, has published in a recent work, the history of two thousand cases treated with, and nearly the same number treated without mercury, with a view of testing the comparative value of the two plans; and he found that in those treated without mercury, the average period of recovery was several days less, and the number affected with secondary symptoms smaller, than was the case with those treated on the other plan. The advocates of mercury may suppose that it was not skilfully employed in these cases, thereby causing it to appear in an unfavourable light; but surely they can no longer urge its *absolute necessity*, after such results as the foregoing have been obtained without it.

A considerable time previous to these experiments in France, a similar practice was tried in England and Scotland, and with equal success. Mr. Rose and Mr. Guthrie have each published a paper on this subject, giving several hundred cases in which cures were effected without mercury. It is worth while to observe, that both these gentlemen were army surgeons, having their patients under their absolute control during the treatment, and under their observation for a long time after they were cured; consequently they could not be deceived as to the remedies employed, or the permanency of the cures. It is also worth while to remember that Mr. Rose's regiment was stationed in London, and his men in the habit, as he tells us, of cohabiting with the lowest orders of prostitutes; consequently some of

his cases must have been of the worst form. Yet all were cured indiscriminately without mercury. Dr. Thompson, the physician to the military hospital in Edinburgh, has also treated a large number of cases successfully without mercury.

But the great and unanswerable mass of testimony on this subject was furnished by the surgeons of the British army, in a general investigation entered into by them in order to settle the question definitively, the result of which is to be found in Hennen's Military Surgery, and in various periodical works. Several thousand cases were treated with and without mercury, and the results were much the same as those before stated to have been obtained in France; the average period for cures without mercury being somewhat the shorter, and the evil consequences attendant on them fewer.

In our own country many physicians have tried the non-mercurial practice, and have found reason to be pleased with the results obtained. Among others, Dr. Harris, formerly a surgeon in the U. S. Navy, and at present a lecturer in the Medical Institute of Philadelphia, has treated a large number of cases successfully, as appears from his paper on this subject in vol. I, of the North American Medical and Surgical Journal. His cures were complete, as appeared from many of his patients having healthy children after marriage.

Such is the evidence on the subject of curing Syphilis without mercury. Much more might easily be collected; but to those who can be convinced by facts, the above are sufficient; and to those who cannot, it is useless to present them. Here, then, I might properly close this paper, having established, to my own satisfaction at least, all I had in view at the commencement, viz. that Syphilis is not a specific disease; that its cause is to be found in the vitiated and acrimonious secretions of the organs of generation; and that it admits of a cure without the use of mercury. I cannot, however, take leave of the subject without observing, that the belief in the specific nature of Syphilis has led physicians into innumerable errors and absurdities. By it they have been led into a belief, that a specific remedy was necessary to cure it; and in their search after this, they have often

lost sight, not only of the principles of therapeutics, but of common sense; for who, with these to guide him, would ever have thought of curing such a disease, or any disease, with such remedies as mezereon, sassafras, sarsaparilla, and a host of others equally inert? By rejecting this unfounded opinion, and considering the disease as the product of ordinary irritation, we have a rational foundation for a rational and successful practice. The same general principles which govern us in the treatment of other diseases, will be applicable in the treatment of this. To cure them, we have only to reduce action when it is too high, to raise it when too low; to maintain a healthy condition of the digestive apparatus, and of the secretions generally, and to remove the causes tending to derange them; and that this plan will cure Syphilis, I know to be a fact. Mercury, as the most powerful agent known in effecting these objects, will most generally and promptly succeed in curing Syphilis; and it is precisely because it possesses these powers that it will succeed; not by reason of any *specific* agency which it exerts in *neutralizing* the virus, or eliminating it from the system.

ART. VII.—*Two Cases of Poisoning with Arsenic.* Communicated by WILLIAM N. MERIWETHER, M. D. of Shelby county, Kentucky.

AMIDST the infinitely diversified duties which devolve upon the votaries of the "healing art," none appears to be more eminently calculated to subserve the great purposes and ends of the profession, than the one which relates to the full and faithfull record of all cases of extraordinary character, which are so frequently met with in the practice of physic. This being done, it then becomes the indispensible duty of the philanthropist, to give such cases publicity, through some accessible medium, in order that his professional brethren may receive

all the benefits resulting from such a source. Influenced in some measure, by a sense of duty, arising from the above considerations, I have been induced to publish the two following cases of arsenical poisoning, which occurred within the circle of my practice, during the last summer. In doing this, I am fully aware of the fact, that the cases do not possess, in general character, any thing intrinsically remarkable; especially as it relates to the local and constitutional influence of the deadly poison, on the systems of my unfortunate patients; but the immense quantity which was certainly taken, in each case, without producing immediate dissolution, is most assuredly calculated to excite, in the mind of the physician, and medical jurist, emotions of interest and astonishment.

The facts, which will be detailed, relative to the quantity taken in each case, were deduced from plain, though unequivocal calculations, the enumeration of which at present would be entirely irrelevant to our main purpose.

About the 10th of July last, I was called in great haste to the family of a neighbouring friend, living about one hundred yards distant from my residence. On my arrival I found Dr. R. A. aged 28, and his mother, 64 years, in the most deplorable condition imaginable. It is perhaps expedient here for me to remark, that not more than fifty minutes had elapsed, from the time the poison was swallowed, until I arrived at the house; immediately upon my approaching the beds of those pitiable victims, I discovered that no satisfactory intelligence could possibly be derived from either of them; by reference, however, to an intelligent female of the family, I ascertained that within five minutes after having supped partly on biscuit, which was the article of diet the poison was concealed in, they commenced a simultaneous complaint of the following symptoms.

A burning sensation pervading the tongue, fauces and throat, followed immediately by vomiting and purging, which soon increased to violent retching, with complete prostration of all the voluntary powers of the system. In this state I found the patients, totally incapable of moving themselves in any way, or of uttering a solitary word, and bathed in a cold, clammy

sweat, which, in the youngest subject, ran in uninterrupted streams down his temples, while his head was suspended over the bedside, in the act of vomiting. The dejections from the bowels, and the matter thrown up by emesis, assumed a dark mucous and acrimonious character; the pulse, in both cases, was free, soft and powerless; no pain was complained of, for the first eight or ten hours.

In order to continue the history of these cases, somewhat methodically, it becomes necessary that I should separate them, inasmuch as the subsequent symptoms became entirely different: and first of the old lady's case, which eventuated fatally.

In this case the purging, which was in the onset very urgent indeed, after eight or ten hours, ceased entirely, leaving the bowels in a remarkably torpid condition; the vomiting, however, continued unchecked, and exceedingly annoying for twenty four or forty eight hours; about this time, another alarming train of symptoms supervened; the bowels now, as above stated, became relentlessly constipated; the vomiting was in some measure suspended, continuing only occasionally, to harass, and distress the patient; burning and pain in the stomach, with intense soreness, and fulness, of the epigastric region; pulse now feeble, and occasionally intermittent; an intolerable sense of heat, thirst, and restlessness, amounting to complete jactitation; tongue moist and dark; a cold, shrunk and clammy skin; ophthalmia tarsi; bloody discharges of acrid mucus from the mouth and nose; general œdema, with purple blotches on various parts of the body. The sufferings of the patient became finally excruciating beyond all description, and on the ninth day, immense discharges of blood from the bowels came on, with delirium, and screaming, and the sable curtain of death dropped over the melancholy scene.

In the young man's case, after the subsidence of the artificial cholera, which continued its influence for several hours, the patient fell asleep, and slept with tolerable quietude, for some time. In this case, no pain was experienced, for the first eight or ten hours; but subsequently, for two weeks, gripings, with colourless evacuations from the bowels were almost incessant,

with asthmatic breathing, palpitation of the heart, dry harassing cough, and universal œdema of the cellular tissue; a bloody discharge of acrid mucus from the nares, and soreness and tumefaction of the eye-lids. These symptoms were gradually relieved, by strict attention to diet, and the occasional use of calomel in scruple doses.

It appears almost useless for me to detail, systematically, the medical treatment in these cases: let it answer our present purpose to say, that in the first instance, the curative indications were, to relieve gastric irritation and sustain the sinking powers of vitality. This was done by the various preparations of opium; fomentations, warm toddy, &c. subsequently: in the old lady's case, calomel, opium and blisters constituted the leading features in the curative plan; the age and situation of this patient altogether prohibited the use of the lancet, for the whole system was so completely shattered from the deleterious influence of the agent, in the first instance, that no reaction whatever ensued; while it was difficult to sustain, to say nothing of abstraction from the vital powers by venesection.

We have every reason to believe, from an estimate made from correct data, that each biscuit contained at a moderate calculation, thirty grains of the poison; which would make the quantity taken in the one case which eventuated favourably ninety, and the other which proved fatal, forty five grains.

The following are a series of experiments which were instituted, in order to ascertain, with certainty, the exact nature of the baneful agent. A part of the dough, of which the biscuits were made, was dried and pulverized; twenty or thirty grains of this powder were put into a pint of rain-water, to which were added, six or eight grains of carbonate of potash; ten or fifteen grains of the oxide of arsenic, with the above quantity of the carbonate of potash, were put into another pint of rain-water; both parcels were subjected to slight ebullition and then filtered.

Experiment 1*st.*—A small quantity of the suspected fluid and of the arsenical solution, were put into separate wine-glasses; the surface of each was then touched with a stick of nitrate

of silver, which in both cases produced a copious orange precipitate, which when dried on a filter, could not be distinguished.

Experiment 2d.—Two slips of white paper were wetted in each solution, (the arsenical and the suspected) and a stick of lunar caustic drawn over both of them, which produced rough yellow lines, precisely similar, remaining permanent for several hours.

Experiment 3d.—To a small quantity of each of these solutions, in separate wine-glasses, were added a few drops of a solution of ammoniacal copper, which immediately produced an abundant precipitate of a fine lively green colour, not distinguishable when viewed by transmitted light.

Experiment 4th.—Sulphuretted hydrogen gas was passed through the real arsenical and a portion of the suspected fluid, in separate vessels, which changed both to a yellow colour, but produced no precipitate in either.

Experiment 5th.—Six grains of the suspected powder were put into a small glass tube, hermetically sealed at one end, with three times its weight of black flux; the bottom of the tube was then urged to intense heat, with the lamp and blow-pipe. The peculiar garlic odour of arsenic was immediately recognized by all present; but the tube broke before the metal was volatilized. A like experiment was tried with pure oxide of arsenic, in which the peculiar odour was constantly distinguishable; but the tube in this experiment broke also, before the heat was sufficiently intense to de-oxidize the metal. The tubes in the above experiments should have been coated with clay, which enables them (as I have since learned) to bear an intense degree of heat.

Some other experiments might have been performed; but the results of those which have already been made, would place all others, if not in a nugatory, at least in a collateral point of view, where the evidence was already so overwhelming.

ART. VIII.—*Two cases of Extensive Imposthumation, terminating singularly. By* Dr. THOMAS P. HEREFORD, *of Haymarket, Virginia. Communicated in a letter to one of the Editors.*

DEAR SIR:

I HAVE determined to communicate to you the outlines of a couple of singular cases of Psoas Abscess; at least they appear so to me, as I have never in the whole course of my reading and practice, seen any that ever terminated in a manner so remarkable. If you should view them in the same light that I do, and deem them worthy of a place in the Transylvania Journal, you can publish them with any remarks that you may think proper to make.

About the 1st of March 1824, I was called upon, in association with Dr. Isaac Henry, formerly U. S. Surgeon, attached to the Constellation, to visit John Chappell of this county. We found him labouring under a high symptomatic fever, arising from violent local inflammation and pain in the lumbar region. He attributed the affection to great exposure during the winter season, and to straining and exerting himself in lifting heavy logs &c. The first symptoms of the disease were an uneasy sensation in the loins, with some inconvenience or difficulty in travelling about on foot; but he still continued to pursue his occupation, which was that of a labouring farmer, for some weeks before he finally yielded and took to bed. We depleted him powerfully by the blood-vessels, bowels, and pores. Nothwithstanding we reduced him much in this way, the pain and constitutional irritation were so severe, that he became almost frantic. We had then to give him large doses of pulv. Dover. or heavy doses of camphor and opium, and to employ rubefacient and anodyne liniments, with blisters over the seat of his sufferings. But all would not do; the relief was only slight and transient. He continued to suffer more or less in this manner

until about the last of the month, when a tumour made its appearance, and gradually diffused itself over the whole of the lower part of the back, and acquired such an enormous size as greatly to disfigure him. After making in vain every effort to discuss the tumour, as I was then alone in the management of the case, I resolved to advance the suppurative process, and to open the abscess as soon as possible; but about the time at which I expected to operate, to my unutterable astonishment, his wife informed me it had broke, and was discharging itself by the rectum. The tumefaction began to subside from that instant, and he continued to discharge pus through the bowels for several days, as much as half a pint at a time, until he must have evacuated some quarts, when the discharges continued to diminish and finally stopt, and never reappeared. Whereupon the swelling slowly augmented, and ultimately became as large as ever. He lingered in this state through all the month of April and until the 9th of May, having gone through a repetition of the treatment formerly adopted, with the exception of any active evacuations which his reduced state entirely forbade.

On that day I made a free opening into the tumour with a common scalpel, and discharged a large quantity of purulent matter. The abscess run, and healed kindly, with nothing more than simple dressings; and by the 22d of May he was well, and remains so ever since. Prior to this illness, he had generally enjoyed good health, was a man of temperate habits, rather muscular and athletic; about 30 years of age.

On the 6th of November 1828, I visited Arthur, a sprightly boy of 13, the son of Mrs. Dorcas Blackwell, of the adjoining county of Fauquier. About a month previously, he went to a neighbour's and engaged in a romp with some wild boys, who pulled and jerked him about with great violence by the heels. The next day on his return home, he felt, in walking, some uneasiness in the neighbourhood of his right hip; and not long after reaching home, he had a chill with soreness; and in two days after, severe pain in the glutæi muscles, posterior to the hip-joint, extending sometimes to the thigh, with stiffness and soreness of the limb. In about two weeks after this, the inguinal glands

became swelled, sore, and painful. In another week the limb contracted. For about four weeks he screamed day and night with the pain, and his mother said she thought he would go into fits. The parts about the hip were but slightly tumefied, with little or no discoloration. The treatment prior to my arrival had consisted principally of slight depletion with salts, &c. cold saturnine lotions, and cold vinegar; which, according to his mother's account, aggravated every symptom. I thought proper to exchange these for warm fomentations and discutient liniments, with an anodyne of opium and camphor, daily. He quickly derived relief from this plan, and never had a recurrence of acute pain. The slight swelling of the parts, and nearly all the soreness, having yielded to these remedial agents, I was induced to suppose that he was almost well; but most suddenly and unexpectedly a tumour made its appearance in the groin, or under Poupart's ligament. It grew larger every day, the matter gradually descending under the fascia lata femoris, until it nearly reached the middle of the thigh, where it seemed to halt, and greatly puffed the anterior and inner part of the thigh, showing a tendency to point, but soon diffused itself over most of the superior part of the limb, or where the investing aponeurotic expansion is thinnest; and by turning the member over, or flexing the thigh on the pelvis, the matter would shift its situation, and sometimes recede into the pelvis. Knowing that there was an immense quantity of pus to be discharged in some way, I ordered warm poultices, with the view of softening and attenuating the integuments, and then making a free incision into it, for the escape of the matter. About the time that I supposed the abscess had arrived at this stage, I visited him to perform the operation; but it was rendered perfectly unnecessary, by an outlet that the resources of nature had instituted: he was discharging the contents of the abscess copiously by the urinary passages, sometimes with the urine and sometimes *per se*. He now began to recruit rapidly; for he was greatly emaciated, and worn down. His appetite, which had been impaired, now returned with a keenness that had to be restrained. The whole of the matter run off in about one week, which made the seventh week of his illness.

His mother says "he discharged a gallon;" but I myself think it probable the quantity was much larger.

The contraction of the flexor muscles of the leg was so rigid, that apprehensions of permanent deformity were entertained; but by repeated efforts at extension, frictions, and warm oily applications, the muscles soon regained their elastic power; and two weeks before Christmas he was walking about. At this time he is a fine active boy, free from any complaint whatever.

ART. IX.—*Cases of Milk Sickness, with some remarks on the Treatment, by* JOHN TERRELL LEWIS M. D. of Lexington, Kentucky.*

CASE 1*st.*—Mr. J. moved to Hardinsburg Kentucky, in the spring, of 1822. He used the milk of a cow, which was allowed to run at large, and soon after his arrival, was attacked with milk sickness, from which he recovered. To prevent a second attack, he confined his cow in an old field, through which an old fence-row ran, much grown up with shrubbery. In a short time, he again felt symptoms of a second attack, and immediately turned his cow and calf together, which had before been separately confined. In a few hours the calf sickened, and finally died of the *trembles*. Mr. J. being of rather indolent habits, and not having taken much of the poisoned milk, had time to use medicines, before the disease was actively manifested. He did so, and was relieved of the symptoms. This precaution was resorted to by him in two instances afterwards, in both of which the calf was affected with trembles, avoiding an attack himself by the timely administration of an active ca-

*Extracted from an Inaugural Dissertation, submitted to the examination of the President, Trustees and Medical Professors of Transylvania University; for the degree of Doctor of Medicine, on the 20th of February 1829.

thartic. The truth of this case may be relied on, and requires no comment.

Case 2d.—Mr. D. J. Lewis, who lived in the neighbourhood of Hardinsburg Kentucky, had always kept his stock confined in pastures, until the summer of 1826, and until that time, no case of milk sickness had occurred in his family, which amounted to about fifty in number. The summer of 1826, was remarkably dry, and owing to this circumstance, many farmers turned their cattle into the woods. This season the disease was quite prevalent. In the neighbourhood of Mr. L. there was a small slip of *Barrens*, in which grew an abundance of fine grass, and from its scarcity in the pastures, he was induced to have his stock driven by a servant to this place, every morning. The servant became negligent, and allowed them to get some four or five miles from home, on the head of Clover Creek, which neighbourhood is celebrated for the annual occurrence of many cases of the disease. The family, unconscious of this circumstance, used the milk of the cows when they returned, and *seventeen* of them were attacked, almost simultaneously, with milk sickness, of which Mr. and Mrs. L died. Here are seventeen persons, all attacked with the same disease, in a few hours after using the milk of cows, which had fed in a milk-sick district of country, some more and others less violently; the different cases being modified by temperament, and the quantity of poisoned milk used. About thirty of the family finally had the disease, but only those who used the milk. Some of the cattle died, and many of them were affected with trembles, at the same time that the family were sick. Those which recovered, lost nearly all their hair.

No case of fever had occurred in Mr. L's family, while living at that place. The house is situated on a very considerable eminence, at the foot of which is a fine lasting spring, and in the neighbourhood there are no ponds, or marshes at any season of the year; in fine it is one of the most desirable situations, in that section of the State.

*Case 3d.**—Mr. Chamberlain, a healthy labouring farmer,

*This case was furnished me by a medical friend.

who lived ten miles on this side of Cincinnati, on the Dry Ridge road, procured the liver of a young beef from his neighbour Mr. Pelly. It was eaten by his family, every one of which, (four in number) were immediately attacked with Milk-Sickness. His children recovered, but he and his wife both died, within twenty-four hours of each other. It was afterwards discovered, that the beef had been affected with trembles a few days before it was butchered, having been seen in this situation, by one of Mr. P's neighbours.

Case 4th.—This case was furnished me by Mr. R. a member of the medical class. That gentleman does not believe in the vegetable origin of the disease, therefore could have no inducement to misrepresent the facts.

The stock of a Mr. Townsend, who lived near Winchester, Tennessee, strayed some distance from home, in the fall of 1819. He was compelled to drive them through a Milk-sick district of country, in returning. One of his steers complained slightly before getting home, but after resting, it became to all appearance well. Soon after his arrival at home, Mr. T. butchered this steer, and sent it to market, reserving only a small portion for his family. On the same day that the beef was butchered, a portion of it was made into steak, of which himself and family ate heartily. Mr. T. expired at the table while eating, his wife soon after; and in a few days one of his daughters. Every individual, who ate of this beef, was more or less violently attacked, with Milk-sickness. It is worthy of remark, that the steak eaten by Mr. T's family, was remarkably rare.

The following cases prove satisfactorily, that persons, who keep their stock confined in pastures, are not attacked with Milk-sickness. Why such individuals are rendered secure by this precaution, is easily determined. It is true that some exceptions have been given; but it may be relied on that they are the result of misconception, or wilful misrepresentation.

Case 5th.—Capt. A. living in the town of Hardinsburg Kentucky, let his milch cows run out, in the summer of 1819. His family were attacked soon after with milk-sickness. No case of the disease had occurred in his family, until that time, nor has

there since, as his stock have been constantly confined to pastures.

Case 6th.—Mr. P. moved to Hardinsburg in the fall of 1819. He had three attacks of milk-sickness, while his cows ran in the woods. He has since that time confined them to pastures, and there has consequently been no recurrence of the disease.

Case 7th.—Mr. M. of the same neighbourhood, had an attack of this disease, in the fall of 1822. His stock ran out until the following spring, at which time he experienced a second attack, of which he was near dying. No case of the disease has occurred in his family since that time, his milch-cows being confined to pastures.

It might be inferred from the very aggravated character of milk-sickness, that its successful treatment, would require great discrimination and experience; but it is not the fact. One indication answered in the early stage of the disease, and all the distressing symptoms subside with astonishing rapidity. This is the evacuation of the bowels, which is as difficult, as it is desirable. It then becomes a question of importance, what remedies should be used to obtain this end? Every one will admit with me that they should be prompt and active, and such as would stimulate least, either by quantity or quality, to answer the indication in view. The medicine most in use in the southern part of this state, is a strong infusion of senna, given as often as practicable. A large proportion of it will be thrown up in vomiting, but by perseverance in its use, a sufficient quantity will pass the pyloric orifice of the stomach, (assisted by injections,) to produce an operation. This medicine is objectionable on account of its nauseating qualities, and the large amount which must necessarily be given. The remedy least exceptionable and most neglected is, in my humble opinion, calomel. This can be administered in large doses, in a small compass. Its great specific gravity, to say nothing of its specific tendency, also makes it a remedy peculiarly well adapted to the treatment of the disease. The bowels should be kept under the impress of some mild laxative, for a week or ten days, after all the symptoms have subsided. In protracted ca-

ses, when there is great debility, some gently stimulating tonic might be admissible, but should be used with great caution. Nothing further will be necessary, but attention to diet and exercise, an abuse of either being extremely hazardous.

ART. X.—*Case of Gonorrhœa cured by injections of the Balsam Copaiva. By* Elijah Coons, M. D. *of Tuscumbia, Alabama.*

A GENTLEMAN applied to me for advice in 1827, who was affected with chronic inflammation of the urethra. He had, about two years previously, contracted gonorrhœa, and was advised to use a strong solution of sulphate of zinc and acetate of lead, as an injection, with other remedies, to remove it. Like most young men, when they first become infected with this disorder, he adhered strictly to the directions, believing that he would in this way be relieved in a few days; and in the course of two weeks his physician pronounced him well. He continued, however, to feel an obtuse pain and a tingling sensation at the seat of gonorrhœa; and had an inclination to micturate more frequently than before he contracted the disease. These symptoms, with slight pain produced on making water, the discharge of a small quantity of muco-purulent matter, and the occasional appearance of warty excrescences on the glans and prepuce, convinced him that there was some urethral affection remaining. His general health was good. The free use of wine, and indulging in the pleasures of the table, aggravated the symptoms.

All the remedies recommended in the standard practical works that were in my reach, were successively employed without any benefit. I then consulted Professor Dudley, in whose judgment, experience and skill I have the utmost confidence, by letter. His advice was carefully pursued for three months, but without avail.

Supposing there might possibly be slight stricture of the urethra, the bougie was introduced, and several times repeated, and yet my patient was no better.

I now determined upon the use of balsam copaiva as an injection; it was injected four or five times a day, and he was restricted to a spare diet and mucilaginous drinks; his bowels kept soluble with Seidlitz powders. This course was pursued for about two weeks, when the patient thought himself much better. He continued it for a few days longer, and was entirely relieved.

I have since had two similar cases, though of shorter duration than the first. The same course of treatment was pursued in both cases, and with the same happy effect as in the first. Might not the balsam be injected in the early stages of gonorrhœa with advantage?

ART. XI.—*Case of Verminose Disease. By* HENRY M. DOWLING, M. D. Leesburg, Virginia.

ON the evening of the 21st. September, I was requested to visit a coloured girl æt. 12 years, the property of Mr. David Ogden of this place. I found her in considerable pain, with quick pulse, hot skin, and difficult respiration. Her bowels, I was informed, had been obstinately constipated for the last five or six days. About ℥x. of blood were taken from the arm, and a strong dose of jalap and crem. tart. directed to be given immediately.

22d. Medicine had operated but slightly: several worms voided: stools black and fetid: tongue dark and considerably furred. Gave calomel aud jalap ʒj *aa*: This was followed by a dose of jalap and crem. tart. in the afternoon. At night, fever considerably increased: patient complains very much of pain in the region of the stomach: venesection repeated: while the blood was running, she commenced vomiting, and before the arm

could be bound up, called for the close-stool: in being conveyed to this, she fainted, but soon revived, and had a tolerably free evacuation. The fœces had none of the appearance, of bile. After the operation she seemed much better: pain in the epigastrium greatly diminished.

23d. Had two or three discharges from the bowels last night. These were, like the preceding discharge, pale and entirely free of bile. This morning the pulse is very frequent, and a little irregular; skin hot and dry; tongue coated with brown; some difficulty of deglutition; incessant spitting; great thirst. Gave calomel and pulv. rhei ℈j *aa*; and left six powders composed of the usual proportions of jalap, nitre, and tart. antimon. to be taken, one every two hours. Calling at night, and finding there had been no operation from the bowels during the day, I requested that the powders might be suspended (of which four had been taken,) and directed a dose of senna and manna. This was rejected almost as soon as swallowed.

24th. Slept very little last night: pain in the epigastrium was excessively severe: medicine operated once slightly: fœces still continue pale. This morning she complains of soreness of the throat; spits a great deal; saliva frothy; tongue tolerably clean; pulse weak and frequent; skin hot. Gave calomel ℈j Ipecac grs. iv in powder: left six pills of jalap and rhubarb grs. iij *aa* to be taken, two every hour.

25th. Rested pretty well last night: medicine acted freely; no difference, however, in the colour of the evacuations. Seems better this morning; every symptom ameliorated. Eight pills of pulv. rhei and aloes were directed to be given at regular periods through the day, with an occasional diaphoretic interposed.

26th. No operation from the bowels yesterday, nor last night. Patient has more fever, and an aggravation of symptoms generally: thirst is particularly urgent: complains of pain in the abdomen; this is not found to be increased by pressure with the hand. In indicating the seat of pain, she places her hand over the *spleen*: heretofore, she has referred the pain to the *stomach*.

Gave calomel and jalap 3ss *aa*; febrifuge powders were directed to be taken through the day.

27th. Patient rested pretty well last night; had two or three small alvine evacuations: no change to be perceived in the colour of the fœces; like the preceding, they are pale and semi-consistent. This morning there seems to be a mitigation of the symptoms; fever considerably abated. Left six nitrous powders to be given, one every hour.

28th. Appears to day a great deal better; is free of pain; almost free of fever; appetite tolerably good. At her own request, she was conveyed to the kitchen; and in the course of the day indulged freely in beef-soup. Visiting her at night I found her feverish and thirsty; this I attributed mainly to her imprudence in eating; a dose of senna and manna was directed to be given.

29th. Patient spent a very restless night, suffering great pain, which she invariably referred to the stomach. Injections had been resorted to, but without effect. This morning her pulse is feeble; tongue clean; skin rather soft; stomach exceedingly irritable; frequent spitting; difficult deglutition. Supposing that worms were the cause of the patient's present distress, I immediately gave her calomel ℈j gamboge grs. ij in powder; and in half an hour a tea-cup full of a strong infusion of pinkroot.

The latter was rejected, and with it two very large worms. Another ℈j of calomel was exhibited. One ounce of the pink root was taken in the course of the day: almost every dose was succeeded by emesis, but no more worms were ejected.

30th. Patient no better this morning; complains of pain in the umbilical region, and left hypochondrium. No operation from the bowels for the last forty eight hours. Directed a solution of salts and tart. ant. to be given in broken doses, and frequently repeated during the day. Saw her at night: medicine had been given as prescribed; but there was no operation. Two doses of Rhubarb of 3ss each, were directed to be administered, one at bed time, the other two hours after. Both were thrown up almost immediately.

Oct. 1st. Spent a very uncomfortable night; no operation

from the bowels; is very weak and languid this morning; still complains of a pain in the stomach. Directed a dose of castor oil to be given; this was repeated in two hours. P. M. She is greatly tormented with thirst; stomach so irritable as not to retain even a few drops of water. At night the pain in the epigastrium returned with redoubled violence. At 12 o'clock I was called up, and found her in great agony. Her cries were loud and incessant; with almost every breath she begged for water. A large epispastic was immediately applied to the seat of pain; and as there had been no operation from the bowels for the last two or three days, measures were adopted to effect one as early as possible. I remained with the patient until day-break, and in that time gave her ʒss of jalap; a part of the second dose was rejected. Before I left the house, the medicine operated twice, once copiously; on inspection, no difference could be perceived in the colour and quality of these evacuations and those which had occurred throughout; they were pale and almost of a natural consistence.

2d. She seems a little better; has taken some nourishment; tongue, however, is brown, and pulse frequent. Calomel and jalap ʒss *aa* were given, followed by jalap and crem. tart. ʒj *aa.* To day about 10 o'clock patient commenced vomiting a green matter, like fluid verdigris; this continued almost without intermission until next morning, when the matter ejected was changed to a black colour.

3d. She is decidedly worse this morning: extremities cold; pulse gone at the wrist; black vomit; &c. Thinking that while there was life there was hope, I directed the patient's limbs to be rubbed with hot brandy; reapplied the blistering plaister; and gave ʒj. calomel; about an hour afterwards a stimulating enema was administered; this induced a call to the chair; but scarcely was she seated before the powers of life sunk; and a few minutes after being returned to bed, she expired.

Dissection. The case detailed above was one in which I felt a good deal interested. There seemed to be an obscurity in it, which, in my opinion, nothing less than a post mortem examination could remove. From these considerations, I was induced

to propose an examination. The owner of the girl being a gentleman of intelligence, and unaffected by the vulgar prejudices entertained on this subject, his assent was obtained without difficulty. In making the examination I was assisted by my friend Dr. Lee; the result was as follows.

Having removed the integuments, an incision was made in the direction of the linea alba from the sternum to the pubes. The first thing presenting itself, and almost before the incision was terminated, was a large worm (lumbricus teres) that had perforated the intestine, and lay immediately under the umbilicus. Our attention being arrested by this singular discovery, the examination among the intestines was continued, and *three* other lumbrici still larger than the first, were found in different parts of their convolutions. In the *transverse arch of the colon* were two or three perforations, where the worms had, no doubt passed, and from which the fluid contents of the bowels were still issuing. The *Intestines*, upon their anterior surface, were besmeared with a thick yellowish fluid, resembling pus: this possibly may have been fat; or the degenerated glands of the mesentery. There was nothing remarkable either in the size or structure of the liver. The *gall bladder* appeared to be considerably enlarged; and distended with a pale blue fluid: about the middle of its body was a remarkable ring, or contraction, an appearance noticed by Morgagni in his work "De Sedibus &c." The *pancreas* was larger than usual; the extremity attached to the duodenum was very considerably indurated, so much so indeed, that there was some difficulty in dividing it. In the appearance of the *spleen* nothing anomalous presented. The *stomach* contained about two ounces of the same black fluid which the patient had vomited during her last moments. Its *internal* coat was of a dark brown colour, and very much corrugated: the *muscular* coat possessed its natural white and shining appearance. A large lumbricus was found in the cavity. No lesion of any kind was to be seen; the vessels of the mesentery in its upper part were injected, and its glands considerably enlarged. There was nothing unnatural in the appearance of the intestines. In the peritoneum, excepting

where the epispastic had been applied, not the slightest trace of inflammation could be detected.

Note. It is observed, no doubt, that constant pain in the abdomen, and difficulty of moving the bowels, were the prominent features of this case. The question is, are these phenomena sufficiently accounted for by the facts developed on dissection? Are we to believe that the *pain* was occasioned by worms irritating and piercing the intestines; or must we suppose it was the effect of some cause still unknown? Again; is the second feature of the case, viz. difficulty of moving the bowels, to be attributed to the enlarged and indurated pancreas pressing on the duodenum and thereby obstructing the canal; or, to the want of a proper biliary secretion; or must we consider it as an effect of the joint influence of these causes?

ART. XI.—*An Inquiry into the Remote Causes of Autumnal Epidemics. By* WILLIAM AUGUSTUS FICKLIN, M. D. of Jackson, Louisiana.

IT will not be our object in the following inquiry, to notice all those agents to whose action the human system so pliantly yields, and to whose operation it so often falls a victim. That would far transcend our limits, as the operative agents extend almost to infinity. Every object cognizable by the senses, is capable, under circumstances favourable to its action, of producing in the system those phenomena which characterize fever. But few of these are sufficiently extensive in origin and permanent in duration, to affect whole communities and give disease an epidemic character. By many their number has been limited to two: cold and miasmata.

Of cold, and its effects upon the system, we are perhaps at the maximum of knowledge. In the chain of effects from the action of the remote cause to the symptoms of disease, there appears no broken or defective link. On the subject of mias-

mata, we are not so well informed. Its sources, and the circumstances producing it, have been pretty satisfactorily defined. The symptoms which follow its action upon the system, class it among the sedative agents. A darker hue of the blood, and a weakened action of the heart, are the first symptoms observed; and stand related to each other in the opinion of Dr. Cooke* as cause and effect. There is an evident defect in this chain, because it does not explain how this black state of the blood is produced.

To the two abovementioned causes of disease, if the second has an existence, which we doubt, we would add a third, which is carbonic acid gas. Our first aim will be to show, that it is an agent capable of producing the phenomena of fever.

Secondly, That its sources are sufficiently extensive to produce epidemic disease.

Thirdly, Its modus operandi.

Fourthly, We shall attempt to prove, that carbonic acid gas and the malaria, miasmata, or marsh effluvia are one and the same thing.

The first proposition laid down by Dr. Cooke, in his theory of fever, is: "The remote causes of fever weaken the action of the heart." This proposition is too well sustained by observation and experiment to be doubted. If this be true, it consequently follows, that all agents capable of weakening the action of this organ, will, under propitious circumstances, produce fever.

That carbonic acid gas, when respired sufficiently diluted with atmospheric air, so as not to excite spasmodic constriction of the glottis, weakens the action of the heart, is equally well sustained. The effects which followed its respiration, in the hands of Priestly, Withering, Beddoes and Percival, induced those gentlemen to pronounce its action upon the system *sedative*. The language used by Dr. Leonard, of Sandwich, Massachusetts, is unequivocal. He says that the respiration of dilute carbonic acid gas "reduces the frequency of the pulse with as much certainty as digitalis; not unfrequently produces nausea and vomiting. It ultimately impairs the appetite."†

*Theory of Fever, Transylvania Journal of Medicine, vol. I. no. I.

†See Thacher's Dispensatory, appendix, no. iij.

In our own experiments, which are specified under the third head of this inquiry, whether the experiments were made with gas chemically or vitally prepared, the same effects followed; weakened action of the heart and its consequences. These effects are found as invariable and as certain, as though we had, instead of respiring carbonic acid gas, taken nauseating portions of tartar emetic, submitted to blood-letting, or had been placed under the specific influence of digitalis; and at one time, its respiration was continued until the pulse declined in frequency, from 70 to 45 beats in the minute, when vertigo and nausea became too intense to be borne longer, for the sake of experiment.

Carbonic acid gas, as shown above, is capable of weakening the heart's action; consequently it is capable of producing fever. This brings us to a close of the first head of our subject.

Secondly. Under this division of our inquiry, we shall first prove, that its sources are sufficiently extensive to produce epidemic disease; and secondly, that it is necessarily most abundant in the atmosphere at the time when epidemics occur.

To establish the first proposition, nothing more will be necessary than to point out its sources. In the words of Dr. Paris, "Carbonic acid gas is evolved during fermentation, by the decomposition of animal or vegetable matter. It is abundantly given off from the lungs during respiration. Plants yield it in great abundance during the night. It exists also in the atmosphere." (Paris' Medical Chemistry, p. 237.) From this quotation we see that its sources are as extensive as is the habitable globe, and that it may lay man under its baneful influence in whatever quarter destiny may place him.

It is plain, that its limited origin in respiration, can never give rise to general disease, can never affect whole communities and nations; for the minute quantity produced, is soon too dilute with air to make an impression. But under favourable circumstances, it may, and we believe has produced disease, in all those within the range of its action. The circumstances necessary to produce a deleterious accumulation from this source, are imperfect ventilation of a room, with a crowded assembly. Such a state as occurs sometimes in jails, in prison ships, in theatres

and courts of justice. The gas thrown off by the lungs without means of dilution from free ventilation, soon accumulate in fatal excess, and destroys its generators. Such instances occurred at the Old Baily, and the Assizes, recorded by Pringle, p. 289–90–91, Rush's ed.; also at the Black Hole in Calcutta, (Philosoph. Trans. vol. 48, Wilson on Fever, vol. 1, p. 407.) We are well aware that the origin of these diseases has been attributed to animal effluvia, the matter of contagion, etc. But the time has happily passed, when a refutation of opinions so absurd is necessary.

We have said, that at the time epidemic diseases occur, the atmosphere necessarily contains a greater proportion of carbonic acid gas than at any other period. Now for the evidence.

The seasons of disease with us are autumn and winter. These are also the seasons of decomposition in the vegetable kingdom; for at these times a large amount of matter has ceased to obey the laws of vitality, and, consequently, come within the pale of chemical influence. In autumn, heat and moisture, the chemical agents necessary to decomposition, act in most perfect harmony, to produce this effect; which, added to the fact, that the quantity to be acted upon is increased, by laws peculiar to the vegetable economy, makes an increased proportion of the agent at this period a necessary consequence.

It is a fact, too, remarked by chemists, that the atmosphere in autumn, contains the largest proportion of this gas.

In winter it is not presumed that so great a quantity is generated, as in spring or summer. But in these seasons it cannot accumulate in the atmosphere, because carbon is the matter of nutrition for vegetable organization, and every particle which decomposition then evolves, is taken up in the process of vegetation.

Whenever the equilibrium is destroyed, either by the excess evolved, as in autumn, or by a diminution of the quantity used, as in winter, the consequent accumulation manifests its effects upon the human system.

To the operations of cold have the winter diseases been attributed. But in many of the diseases of that period, we dis-

cover symptoms analagous to those present in autumnal epidemics. This analogy has been explained by Dr. Cooke in his truly valuable essay on winter epidemics, (Trans. Jour. p. 53,) as an effect of the autumnal agent, continuing in the system. But we are deceived if the agent is not then present and active in many cases; for it cannot be denied that in our mild climate, there is frequently a sufficiency of warmth and moisture in winter, to complete a decomposition commenced during the autumnal months. A fact long since noticed by physicians, may be used in support of this opinion. It is this: that the coldest winters and the coldest periods of every winter, are most healthy; and it is not until the icy grasp of cold has been unlocked, that disease invades us. It is then that warmth and moisture are in action, and it is then that the pestiferous gas is loosed upon its victims.

Modus operandi. This part of our inquiry is not only essential in rendering perfect the chain of effects, but necessary to establish the quantity of carbonic acid gas which atmospheric air should contain to become deleterious. Respiration and the lungs being the media through which our agent operates, a word upon its uses and their function, is rendered necessary by the conflicting opinions of physiologists on these subjects. The earliest theory was, that the air contained the vital principle, and imparted life to the system through the medium of the lungs, they serving as mere conduits for the transmission of the vital fluid. We are not surprized that such ideas existed, when the sciences of anatomy and physiology were in their infancy. For our fathers saw in respiration, the first and last voluntary act of life. They witnessed the convulsive action and death which followed when "the breath of life" was withheld. All organized matter was endowed with a respiratory apparatus. The phenomena could not be explained by any of the then known laws of the vital economy, and the universality could in no philosophical manner be accounted for. Hence their opinion. Chemistry detected changes wrought in the air itself by respiration. A portion of it was consumed, and immediately oxygen was called the princi-

ple of vitality. All those changes undergone by the blood during its transit through the lungs, were attributed to its agency.

Further experiment proved, that the loss of oxygen was only apparent; and Mr. Lavoisier's finely wrought theory of *oxygenation* fell into merited neglect: but not until it had established the necessity of making the blood a vital fluid. The changes in colour, in temperature, &c. which were before attributed to the absorption of oxygen, were now the result of increased vitality alone, without regard to those chemical changes produced in the blood by the vital action of the lungs. The functions assigned to the lungs by Mr. Lavoisier had scarcely been set aside by experiment, when the *vitalizing* office of the schoolmen took its place. But we beg leave to withhold assent from any office assigned the lungs which has for its object the vitalizing of the blood; because that fluid does not possess vitality.

Of life in the abstract we know nothing, and can only judge of its existence by a series of phenomena which its presence gives origin to. The principal of these phenomena, in the most simple organization, are secretion, nutrition and growth, superadded to which, in the most complex, are sensation, motion and thought. None of these phenomena is the blood capable of exhibiting. Living matter possesses irritability, sensibility, and contractility; properties or laws of whose existence in the blood we have no evidence. Vitality is a principle belonging exclusively to the solids, nature having in no instance associated it with fluidity.

Another reason why we do not believe the blood to possess life, is, that for none of its offices is vitality necessary, and nature does nothing in vain.

The blood, simply as a stimulant, can excite the contractility of the circulatory system, and thereby act as the exciting cause of its own propulsion.

As a fluid, it is the proper vehicle for conveying the nutrient particles of the food, to every part of the system, or for returning to the proper emunctories, its waste and effete matter. These appear to us its only offices, and these offices it can perform without higher claims to vitality than alcohol, or any other stim-

ulating fluid deserves. Then what function do the lungs perform? They *decarbonize* the blood; and the objects for which respiration is destined, are to effect pulmonic circulation, and to give the carbon a gaseous form to facilitate its elimination from the system. That is the only function, and these the only objects, which experiment and observation have detected; and these are ends well worthy of the means. For the blood, after having taken into its mass the products of absorption, and received from the thoracic duct its contents, contains necessarily an excess of carbon, and is no longer an efficient stimulus for the left heart and arteries. Hence circulation would be paralized and death ensue, if the excess was not removed by the lungs, whereby the stimulus is adapted to the tissues with which it comes in contact, and action follows.

From this digression we return to consider the modus operandi of carbonic acid gas.

We have shown above, that the effect produced upon venous blood by the lungs, is to deprive it of the excess of carbon, received from absorption and digestion; and that the effect of decarbonation is arterialization, or the rendering of venous blood into an appropriate stimulus of the left heart and arteries. We have also shown, at an earlier period, that the effect of carbonic acid gas, when applied to the lungs, is to weaken the action of the heart.

Now, if the vigorous action of the heart depends upon a perfect decarbonation of the blood, or in other words upon a due performance of the functions of the lungs, it follows, that any agent weakening the action of the former, when applied to the latter, must impair or destroy its functions in a ratio equal to the effects observed.

That carbonic acid gas impairs or suspends the function of the lungs, we also infer from the following remarks made by Dr. Paris: that, "atmospheric air, after having been once admitted into the lungs, returns charged with 8 or 8.5 per cent. of carbonic acid. If the same portion of air be breathed repeatedly, the quantity of carbonic acid cannot be increased beyond 10 per cent." (Med. Chemistry, p. 262.)

What prevents the proportion of carbon from increasing in a ratio equal to 7 per cent. at every respiration? It is prevented by the influence which the acid formed exerts over the function of the lungs; impairing that function until the proportion amounts to ten per cent, at which time its suspends it entirely.

The following experiments were made to test the correctness of the above inference and conclusion.

Experiment 1. Atmospheric air was admitted into the lungs and expired. It contained 8.5 per cent. of carbonic acid. At the time the experiment was made, the atmosphere of our vicinity contained about 1 or 1.5 per cent. of carbonic acid. In the first respiration then, the lungs secreted from the blood about 7 per cent.

Experiment 2. The air was submitted to two respirations; and upon testing it, was found to contain 9.75 per cent. of carbonic acid. If the first respiration produced 7 per cent. the second gave only 1.25, showing a diminution of 5.75 per cent. of acid.

Experiment 3. Upon a third respiration of the same portion of air, it was found to contain 10.5 per cent. of acid; the lungs in the third respiration producing only .75 per cent. of carbon: the relative products of the first and third respirations being as 7 to .75 per cent.

After the proportion of acid amounted to 10.5 per cent. no number of respirations could increase the quantity.

From the respiration of acid chemically prepared, the following are the results.

Experiment 1. To atmospheric air we added 1. per cent. of acid; and when respired, it contained about 8.5 or 8.75 per cent. Increase 6 or 6.25 per cent.

Experiment 2. Two per cent. was added; and there was but a small increase in the proportion, after respiration.

Experiment 3. In this experiment 4 per cent. was added; and respiration gave 9. Increase 3.5 per cent.

Experiment 4. Six per cent was added. The result was 9.5 after respiration. Increase 2 per cent.

Experiment 5. To atmospheric air we now added 8 per cent.

of acid. The amount upon respiration was 10 per cent. Increase .5 per cent.

Experiment 6. The quantity now added was 9 per cent. That expired was 10.5 per cent. No increase of acid in this experiment.

Experiment 7. We next added 10 per cent. Amount on repiration 11.5 per cent. No increase. The function of the lungs in the two last was suspended: for the proportions mingled with the air, 9 and 10 per cent, added to the quantity which the atmosphere was found to contain at the commencement of the experiments, which was 1.5 per cent. equals the quantity detected at the close of each experiment.

We see by the last set of experiments, that the quantity of carbon secreted from the blood by the lungs, is diminished in a direct ratio, to the quantity of acid in the atmosphere, which confirms the inferences drawn from its effects upon the system, and from the fact stated by Dr. Paris.

The inference drawn from its effects upon the circulation was, that it impaired the decarbonizing function of the lungs; and the correctness of the inference is established by observation and experiment.

This completes the chain of effects, from the first impression of the remote cause, to the symptoms of disease. The remote cause impairs the function of the lungs; the effect of this effect, now becoming a cause, is an excess of carbon, in the blood destined for arterial excitement. The effect of this excess, is weakened action of the heart, and consequent congestion, the proximate cause of disease.

Although it has not been openly avowed, that by an excess of carbon in the blood, febrile phenomena are produced and continued; yet, the practice of the scientific and empirical is a tacit acknowledgement of the fact. To excite the action of the liver, is an object in autumnal disease, which both have in view; and should success not crown their efforts, despair seizes the boldest. In exciting this secretion they decarbonize the blood, and by this means restore the healthy equilibrium of circulation. Hence the reason why all chologogue cathartics are stim-

ulating. All, who have, by calomel or other means, excited biliary secretion, bear testimony to the fact. Nature, in establishing these two decarbonizing and antagonizing organs, the lungs and the liver, has displayed that sagacity which characterizes all her works: for, under the circumstances that surround us, the lungs must often be subjected to the action of agents capable of impairing their functions, and when the injury sustained is partial or of short duration, the liver relieves the system from its effects, by a corresponding increase of action. Hence the frequent diarrhœas of spring and summer. In autumn, however, the impression is too powerful, or too long continued, to be relieved by spontaneous action, and the phenomena of fever follow. But we believe, that by keeping an eye to hepatic secretion, increasing it by appropriate stimuli when the cause of disease exists in greatest force, that all the malignant forms of fever would be rendered mild; and that those who, under ordinary circumstances would be subjects of dysentery, diarrhœa, &c. under the present, would escape entirely.

Fourthly. We now come to the concluding division of our subject, and, as we proposed, shall attempt to establish the identity of carbonic acid and miasmata.

We believe them the same for the following reasons: First, they originate from the same sources, vegetable and animal matter.

Secondly, they are both evolved during decomposition.

Thirdly, the action of both upon the system is alike, sedative.

Fourthly, carbonic acid is *necessarily* present at the time when miasmata is *supposed* to be present from its effects.

Fifthly, they are both produced by respiration; or the diseases that both give rise to, are alike. One of these analogies is indisputably true, and we believe both, for the subjoined reasons.

The causes producing the diseases at the assizes in the Old Baily; in the Black Hole, Calcutta; in jails, and in prison ships, when crowded, cannot possibly have any other origin than in respiration. If it were carbonic acid, the diseases produced were such as occur in autumn, being malignant fever, dysente-

ry, diarrhœa, &c. (See the works referred to on the same subject.) And if it were not carbonic acid, but the malaria, then they have a common origin as well in respiration as decomposition.

Sixthly, They possess similar material properties.

The properties of carbonic acid gas, pointed out by chemistry, are first, a specific gravity greater than atmospheric air, in the proportion of 30 to 46. Secondly, it extinguishes flame. Thirdly, it is fatal to animals. Fourthly, it is absorbed by water.

That miasmata is a dense gas, we have abundant evidence; that it is heavier than atmospheric air, we infer from its affecting the inhabitants of the lower apartments of houses more than those in the rooms more elevated.

That it extinguishes flame, we have testimony sufficient to establish, in Bancrofts Sequel, p. 232–3; also in the Medical Repository vol. iv. no. I. In the latter, the case recorded is, by Dr. Kollock, and of the frigate Gen. Greene; lights would not burn in her hold or cockpit. The diseases it gave rise to, were yellow and bilious fever. In these cases, the contaminated state of the atmosphere has been traced to decomposing stores, cargo, timbers, &c.

The malaria, when concentrated, is instantly fatal to animal life. An instance is recorded by Dr. Trotter, (Med. Naut. p. 324,) in which this effect was produced. A large amount of coffee had fermented in the hold of an Indiaman, and many of her crew upon opening the hatchway fell dead upon the spot.

The malaria is absorbed by water. This we infer from the fact, that the inhabitants of miasmatic districts, who live immediately on the margin of large bodies of water, are less the subjects of its influence, than those higher, or further removed. In the fatal epidemics which have visited Natchez, the inhabitants "under the hill" are not so frequently or so severely attacked, as those "on the hill;" because the watery particles arising from the Mississippi envelope those in the valley and absorb the miasmata, while those above are without the sphere of this watery envelope, and are exposed unprotected. It is further re-

marked, that a shower of rain impedes the progress of epidemics, from the same cause.

To conclude: we have shown that carbonic acid gas and the malaria, arise from the same sources, and are produced by the same circumstances: that when applied to the system they produce the same proximate and remote effects, and they both possess the same material properties. Upon these points of coincidence, rests our opinion of their identity, whether true or false, future investigation will prove.

ART. XII.—*Essay on Phlegmasia Dolens. By* John E. Cooke, M. D. &c. *Read before the Lexington Medical Society, and published at their request.*

Gentlemen:

THE subject to which I wish to call your attention this evening is of a practical nature and of very great interest. I allude to the disease called phlegmasia dolens.

The subjects of this affection are almost exclusively women: the part affected in almost every case, the leg.

The appearance of the part, in the state in which it is generally first shown to the practitioner, is very singular. The limb is extremely large, and has the appearance of being stretched to the utmost. It is quite pale, and even white. It shines, and has, when in the utmost state of distension, a transparent cast as if light was passing through it, above the opaque subjacent parts. It is is excessively sore when touched in any part, and there is violent pain in particular spots; sometimes at the front of the top of the thigh; sometimes on the inner side of the thigh, a little below the middle. The pain is often in the calf of the leg; and in some rare instances near the ankle. In this state of the disease, the patient cannot bear sufficient pressure to cause a pitting of the surface; and the tension of the

skin is such as to convey the idea that a depression could not be made in it; and when a slight one is made, on removing the pressure the depression disappears.

The disease as has just been described fell under my notice many years ago; and with every effort I could make, aided by the advice of a gentleman of experience, whom I occasionally consulted, it ran on to twelve or thirteen weeks; and the recovery was very slow, painful, and imperfect. This was the case perhaps in every instance for some years. Nothing done seemed to have any material effect. Blistering produced more than any other remedy, and was chiefly relied on; but even that remedy was not used to any extent. In this state of things, I was one day called to see a woman in child-bed, who complained of a pain in the groin, precisely where the glands are seated. On feeling them, I found a small hard tumour; on pressing which, the pain became more severe. I imagined it to be one of those slight affections of the glands which people commonly call a kernel, and told her so. She was advised, however, as she complained considerably, to take a cathartic; and afterwards, as the pain was not thereby removed, to apply a blistering plaister over the spot. The pain by means of the two remedies abated; but she still complained, and in a short time it began again to increase. Her leg soon after began to swell, but the swelling was such as we commonly call œdematous. Pressure made pits in the tumour, which continued for some time after it was removed. About this time a neighbour of hers, who had had the swelled leg shortly before under my care, visited the patient. On seeing the leg, she immediately pronounced it to be the same disease as that which had so long afflicted her. As, however, the tumour in phlegmasia dolens, in the distended state in which the leg is commonly shown to the physician, is elastic, I was persuaded that this case, in which the swelling received and retained the impression of the finger, could not be that disease; and declared she was in an error. She nevertheless maintained her opinion, asserting that her leg had at first been in the very same state that this was. We could not agree at that time; but in a few days the question was decided against me;

and I was compelled to admit, that the disease really was phlegmasia dolens. It ran the usual course: the patient was confined to bed for weeks and months, by the swelling first of one leg and then the other.

A defeat is sometimes of greater advantage to a man than a victory. It was so in this. I immediately acknowledged that I had been wrong, and accounted for it in this way: that I never before had seen a case until the disease was fully formed, and therefore did not recognize it in its commencing symptoms, which I then for the first time observed. The benefit derived was, however, the inference drawn from what was in this case observed. The disease evidently commenced with such pain &c. increasing by pressure, as to indicate the propriety of evacuating in order to cure it. This course was immediately determined on as proper to be followed in the next case that might occur.

The first case after this, was that of a lady who had had for some months before a cough and bloody expectoration. On account of the last named symptoms, her death was generally considered as certain in a short time. She was purged with mercurial cathartics repeatedly, and blistered once; and recovered entirely in a short time from the phlegmasia dolens: and by the occasional use of the same remedies, with bleeding, she was cured of the cough and expectoration, and became an unusually hearty woman, and continues so five years after her recovery. In this case it is to be observed, that the disease was not allowed to progress considerably before it was arrested. But the symptoms, except the great distension, were all clearly marked. The patient was well in a week or ten days; and a woman who had suffered under the disease three months, visited her to inquire into the case, astonished at the rapidity of her recovery.

Encouraged by this success, the same plan has been followed in the treatment of every case which has occurred in my practice since that time. The subsequent cases were, however, treated with more vigour, and the result was proportionally decisive and beneficial. In one of the last cases, the patient was

a small woman, of dark complexion, and great natural intelligence. She had, in child-bed, this swelling in her right leg, with great pain on the inside of her thigh. She was bled, purged actively, and blistered freely; and was completely relieved in a very few days. Some days after, the left leg swelled. A blistering plaister relieved it almost entirely. The pain, however, though driven from the inner part of the thigh, affected successively two or three parts of the limb below. It was followed up by blistering every part, and she by this means, together with the use of mercurial cathartics to keep the bowels in a loose state, entirely recovered in two or three weeks of this affection of both legs.

In another instance, the effect of the most active treatment, although the patient had been for some months previous to the attack in a very debilitated state, was most remarkable.

This patient was a very small woman. She had been in bad health for a considerable time. She had had symptoms of general dropsy, with leucorrhœa, disordered stomach, &c. A few days after being delivered, she complained of excessive pain in the top of the thigh, and on the inner side of it, some little distance above the knee, with swelling of the leg. Her sister, a very intelligent lady, who had seen the disease, pronounced it at once to be the swelled leg. But nevertheless relief was not sought for several days by any other means than such as they supposed might be beneficial.

When called, I found her very feeble, but with an active pulse; with great pain in the limb, excessive soreness on touching it, and considerable swelling. She was bled twice, blistered twice, and purged every day freely with pills of aloes, jalap, and calomel. When she was bled the second time, she was very weak, and was not allowed to sit up in bed for fear of fainting.

Under this treatment, the pain was gone in two days; the swelling in a week.

Except the cases treated in this manner, I never knew one which did not continue for many weeks, generally ten or twelve. In every case thus treated relief has been immediate, and recovery rapid; no case having continued more than two weeks, and

some cases which commenced with such severity as to give reason to apprehend protracted illness, being completely cured in one week; notwithstanding that the previous debilitated and diseased condition of the patient in two of the cases, was such that a protracted, painful illness, would in all human probability have carried them off.

Having stated the mode which in my hands has been thus far completely successful in the treatment of this disease, together with the manner in which I was led to the adoption of this mode of treatment, it will readily occur to the Society that the state of the glands observed particularly in the first patient, is an important circumstance, and may lead to the knowledge of the manner in which the subsequent symptoms are produced. I shall now endeavour to trace out the nature of this disease, or the state of the limb affected.

1. It cannot be questioned that the tumour is produced by the retention of a fluid in the limb. Nothing else can be imagined in so short a time to be added to the limb and to be taken away again. Indeed it needs not words to satisfy you on this point.

2. This fluid must necessarily be either that contained in the arteries and veins, or that which is poured out of them into the cavity of the cellular membrane, and contained in it and the lymphatic and absorbent vessels. Of this no proof need be offered. The fluid can exist no where else than in one or the other of these cavities.

3. The tumour cannot be produced by an accumulation of blood in the blood-vessels of the part, because such an accumulation would inevitably give to the limb a red or livid colour, whereas it is remarkably pale, and shining. If the arm be tied up for the purpose of bleeding, in a few moments the accumulation of blood in its vessels, gives to it a red or livid colour; how much more would this be the case where the accumulation continues for many weeks. No instance can be produced in which blood accumulates in the vessels of a part, in sufficient quantity to produce acute pain and swelling, without producing a red colour of the surface.

It cannot therefore be by accumulation of blood in the blood-vessels of the limb that this tumour is produced.

4. The swelling must therefore be caused by fluid poured from the blood-vessels into the cavity of the cellular membrane, and contained in it, or in the lymphatic or absorbent vessels, or in both.

5. This being the case, the question arises, Is the fluid contained in the cavities of the cellular membrane, or in the lymphatic vessels, or in both?

6. An accumulation of lymph in the cavities of the cellular membrane is readily distinguished. In consequence of the communication between the cells of the cellular membrane, a slight pressure drives the fluid from the part pressed; and as this fluid moves slowly, some time elapses before it returns and fills up the depression. This continuance of the depression has been universally considered as the test of the presence of fluid in the cells of the cellular membrane.

7. This being the case, we should be led to conclude, on examining a limb in the advanced stage of phlegmasia dolens, that the swelling is not of that kind: because it will not allow of more than a very slight depression; because the depression does not continue, the part returning instantly to its former state on removing the pressure; and because it gives considerable pain to make pressure, which is not the case in œdematous swellings.

8. If not produced by fluid in the cavities of the cellular membrane, it must be by fluid in the lymphatics or absorbent vessels; and all the circumstances strongly confirm this conclusion.

9. The lymphatics are tubes with very many valves; and as several of these are found, in some parts, in the compass of an inch, these tubes may be considered as divided by them into numberless cells, not however freely communicating in every direction; but each lymphatic consisting of a range of cells, capable of allowing a fluid to pass but one way, viz. towards the subclavian vein, and through those bodies called the glands of the absorbent system.

10. Let us consider what must be the consequence of an obstruction of the passage of the fluid of the absorbents through these glands.

11. The first consequence is obviously an accumulation of fluid in these vessels. This must continually increase; because while the action of the heart continues, secretion must continue, and consequently the absorption of the fluid secreted into the cavities of the cellular membrane, which every moment adds to the quantity in the obstructed absorbents.

12. It is obvious therefore that from such an obstruction must arise all the symptoms which flow from a gradually increasing distension of sensible parts; soreness and finally pain; tumour gradually increasing and finally excessive; no yielding to pressure to any extent, and no continued depression gradually filling up again, because the fluid forming the tumour is not to be displaced, being prevented from passing onward by the obstruction in the glands, and backward by the valves.

13. When the absorbents are filled as full as they can be, the fluid secreted into the cavities of the cellular membrane, accumulates in them, and thus is the evil increased to the utmost.

14. It may be observed in addition to what has been said, that this view of the cause of the disease is confirmed by the inefficacy of frictions in phlegmasia dolens; the fluid not being able to pass on through the glands.

13. There may be, together with the tumour from accumulation of fluid in the absorbent vessels, swelling from fluid in the cavities of the cellular membrane, arising from some cause producing this, at the same time that the obstruction of the glands produces that. In the first case stated in this essay, there was such a swelling, but it is particularly noted in the two last cases stated, that no continuing depression could be produced by pressure.

REVIEW.

ART. XIII.—*A Treatise of Pathology and Therapeutics.* By John E. Cooke, M. D. Professor of the Theory and Practice of Medicine in Transylvania University. In three volumes. Lexington, 1828.

We consider this an able, original work, and one which is likely to exercise a signal influence on the medical doctrine and practice of the day. In the South and West it bids fair to supersede, in a great degree, all the other works on this particular department of medical science. We will not pretend to predict how long this influence is to endure, nor whether it will not in turn be itself superseded by some more ingenious and popular system. Like the theories which it is designed to subvert, it is the work of a man, and though he appears to have dug deep, and to have reached a solid foundation, and fortified himself by extensive reading and close thinking, it is no detraction from the merits of his work to suppose that he has been occasionally warped by a favourite opinion, and allured, in some instances, into the regions of conjecture. If it contains more truth and fewer errors, if it affords pathological views clearer and more consistent with nature, and indicates a practice safer and more efficient than other works, the author should be satisfied with his labours, and the public will be apt to consider it a good book. But our business is not so much to set the proper value upon the work, as to present our readers with an analysis of its contents; and the author being one of the editors of this Journal we should not deem it discreet to say every thing in its praise that we think it deserves. And we are the more indisposed towards the task of commendation from the knowledge, that our professional brethren in the South and West are independant republicans, who think and act for themselves, and will form their own estimate of the work without the aid of critics.

After a more pertinent preface than we commonly meet with in books, the author commences, in his first chapter, with an account of the attempts that have been made to explain the remote and proximate causes of disease.

Passing in review the principal hypotheses that have been proposed, and showing their insufficiency, he proceeds in his second chapter to speak of the "remote causes of disease, their mode of operating, and their effects on the body." Among these are hunger, excessive bodily exertion, the depressing passions, want of sleep, stimulants used to excess, intense application of the mind, cold, &c. all of which directly or indirectly weaken the action of the heart. "Cold is one of the most common, and is the most immediately active of the remote causes of fever;" but it, with the other causes mentioned, is limited in its operation.

In chapter III still marching under his banner motto, *felix qui potuit rerum cognoscere causas*, he enters upon the investigation of the remote cause of the epidemics of hot weather, and after travelling over immense space, giving line upon line and precept upon precept, he comes to the conclusion that "it is a vapour or gas, produced in marshy places."

"It is evident, therefore, that there is not sufficient ground for believing that the epidemic diseases of hot weather are produced by a gas arising from putrefying animal matter; or by bad drinking water; or by the influence of clay lands; or by water below the surface of the earth; or by moisture alone; or by dryness of the atmosphere alone; or by a rarefied state of the atmosphere; or by comets; but that we have abundant reason to believe these wasting pestilences are the effects of a dense gas the product of the putrefaction of vegetable matter.

"The more closely we investigate the subject, the more certainly we shall find that this doctrine rests on the strong foundation of truth. Most physicians have indeed been convinced of its general truth; yet do many of them inconsistently attribute the minute variations of epidemic diseases to some *unknown* constitution of the air. "More causes of natural things are not to be admitted than are both true and sufficient for explaining their phenomena."* An *unknown* cause ought not therefore to be resorted to for explaining these variations; particularly as it is evident that the various degrees of the power of the cause which produces an epidemic, are not only sufficient to produce minute variations, but *must* produce them.

*Newton.

"The subject is worthy of the fullest investigation; and I shall endeavour to show, by an examination of the topography of the various parts of the earth in which epidemic diseases have appeared, and of the circumstances in which they have arisen, that in every country the epidemics of hot weather are produced by the same cause, the dense gas arising from putrefying vegetable matter."—*Vol.* 1, *p.* 100. 101.

If there is one point in the science of etiology clearly made out, we think it is this. The array of facts presented in this and the succeeding chapter, we should deem sufficient to put to flight the stoutest incredulity.

The 4th Chapter, which extends from p. 102 to 216, is taken up with an examination, and it is a most interesting and satisfactory one—"of the topography of the various parts of the earth in which epidemic diseases have appeared, and of the circumstances in which they have arisen." Our limits will allow us to introduce but a few of the facts. We would refer the skeptical to the work itself.

"In 1796 an epidemic fever excited considerable alarm in Boston. July and August of that year were intensely hot. The disease appeared on the 25th of August. "A very great proportion of the sick were near extensive flats, particularly about the easterly, southerly, and westerly skirts of the town. The place called Oliver's dock, where this disease was most prevalent, was exposed to exhalations from foul substances lodged about the wharves and docks of that quarter, with buildings so constructed as to admit of very imperfect ventilation, and with large numbers of inhabitants crowded together in a small space."* As the cool weather advanced, the mortality was greatly diminished. Dr. Warren, from whose account of the fever this is taken, had almost every fall seen a considerable number of cases similar to those of 1796, not excepting the black vomit, yellow skin, &c.†

"The yellow fever prevailed in Boston in 1798 also. The months of July, August, and part of September, were hot and sultry.‡ The weather was never known to be so uniformly and excessively hot.§ The fever commenced on Stoddard's wharf, situated between Mill-creek and Town dock. The dock is the receptacle of a large sewer, into which, in consequence of its being in the vicinity of Faneuil Hall market, all the offal of the market is thrown.‖ "The market-place is a low sunken part of the town. It is, from situation, the reservoir of every putrid matter flowing in from more elevated parts of the town, and accumulated by every rain. It is surrounded by docks

*Medical Repository, Vol. 1, 139. †Ibid. Vol. 1, p. 140.
‡ Ib. Vol. 2, p. 467, by Dr. Rand. § Ib. p. 392, by S. Brown, M. D.
‖Ibid. p. 467.

of stagnant water, filled with offal, and all manner of noxious matters."*

"The disease also appeared at the latter end of July, at the foot, and on the declivity of the south-east and south sides of Fort Hill, and made a fatal progress. Scarce any family that resided below the summit, on these sides of the hill, escaped the fever. One lost five persons out of six. The south-east, south, and south-west winds prevailed most of the time of the sickness, which wafted upon the inhabitants of those sides of the hill the exhalations from the wharves, stores, and docks, at the foot of it, and from the extensive flats in the skirts of those quarters of the town.

"In the last of September the wind shifted to the west and west-north-west, which conveyed the noxious exhalations from the mill-pond into Back-street. The inhabitants contiguous to the pond, and others, throw every kind of filth into it and the filth of the streets flows into it in every direction. This pond was often drained of its water, so as to leave the mud and putrid substances exposed to the action of the sun. When the fever appeared in this street, the selectmen were requested to order the water to be retained constantly in the pond; which being done, the fever soon ceased in that quarter."†

"Natchez is situated on the east bank of the Mississippi, in north latitude 33 degrees 31 minutes, from one hundred and fifty to two hundred feet above the surface of the river at low water. The shore forms a high precipice, which excludes the view of the river. The surface is very undulating, with a gradual but irregular declivity for about three miles eastward to St. Catharine's creek. "It is sufficiently remote from any swamp on the same side of the river not to be affected by its exhalations. The soil is a rich black mould. Bilious fever was common in the summer and fall, of which occasionally there were a few cases of the highest grade, but previously to the year 1817, the inhabitants were not afflicted with an epidemic fever."

"After the termination of the last war, there was a rapid increase of population. Many improvements were made, buildings erected, and arrangements made and partially executed, to reduce to a convenient level the irregular surface of the site of the town. By this means ponds were formed in many parts of the town, and the natural course of the water being stopped, cellars in the low places were often partly filled; the back yards of many houses also, being thrown below the level of the street, were often wet and became the receptacle of filth of every kind.

"The year after the peace, 1816, was very dry and healthy. The year, 1817, was wet and sickly. The year 1818 dry and healthy. The year 1819 wetter and more sickly than 1817. The following table, taken

*Ibid. p. 390. †Medical Repository, Vol. 1, p. 470,

from the one given by Dr. Perlee, will place the whole in a clear light at one glance. The rain is stated in 225th parts of an inch.

	Rain from the 1st of May till the last of August, four months.	Mean temperature, June to September, four months.	Deaths.
1816	1961	79 1-4°	no epidemic.
1817	3285	77°	about 300.
1818	1919	76 3-4°	no epidemic.
1819	5362	77 3-4°	number not mentioned; but stated to be the most sickly year ever known.

Of the rain which fell in the four months of 1819, very near four-fifths fell in July and August, and filled most of the cellars, and all the low places of the town. At the subsidence of the flood, hundreds of acres along St. Catharine's creek, were covered with sediment from a few inches to several feet in depth, which afterwards becoming dry and cracking open, the putrefying substances underneath emitted most offensive exhalations. Great sickness and mortality occurred all along the course of this creek. The epidemic destroyed horses, cows, dogs, poultry, and the deer of the forest."—*Vol* 1, *pp.* 123, 4, 5.

"Thus excessive rain produces in dry places sickness, and in wet places health, while excessive drought produces in dry places health and in wet places sickness."—*Vol.* 1, *p.* 180.

Chapter V is an attempt to show that the "same cause produces the epidemic diseases of Asia, Africa, and Europe," and we think it is attended with success. The survey exhibits extensive learning and research in the author. We will give a few of his examples.

"The Delta of the Ganges, a part of the province of Bengal, traversed by the numerous mouths of that river, commences two hundred and twenty miles from the sea, or three hundred by the windings of the river, and extends about two hundred miles in width at the lower part. That part next the sea, called the Sunderbunds, is a labyrinth of rivers, so that there is a complete inland navigation throughout and across the lower part of the Delta. This portion of the country, as well as a considerable one north of it, is as flat as the Delta of Egypt. The rivers continually bring down large quantities of mud and sand, and evidently encroach on the sea. There is not any substance so coarse as gravel in all the Delta, or nearer the sea than four hundred miles. In all the sections of its numerous creeks and rivers, nothing appears but sand and black mould. There is on this rich soil a luxuriance of vegetation almost unknown to any other part of the globe."

"The rainy season continues from the beginning of June to September and October. By the latter end of July, all the lower parts of Bengal adjacent to the Ganges, are overflowed, the inundation being

more than an hundred miles wide. Towards the middle of August the waters begin to subside."—*Vol.* 1, *pp.* 217, 218.

"In this rich country, laden with vegetation, soaked with continued rains, and exposed to excessive heat from the perpendicular rays of the sun, fevers and fluxes annually carry off immense numbers in July, August, September, and October."*—*Vol.* 1, *p.* 218.

"The country about Bassora, on the river Tigris, is so low that it is only prevented from being inundated by a stone embankment. The Arabs were at that time in the habit of revenging themselves for any injury done them by the Turks of Bassora, by breaking down the banks of the river, and thus laying the country under water. The consequence was a mortal epidemic. On one of these occasions twelve or fourteen thousand of the inhabitants died; and not above two or three of the Europeans settled there escaped with life."—*Vol.* 1, *p.* 223.

"Pringle says before the Romans knew how to let off the water standing around the city, it was exceedingly sickly; but when drains and sewers were made it became more healthy, and only the lower and wetter places of Latium remained sickly. When the city fell into the hands of the Goths, the drains being stopped, and the aqueducts cut, the Roman territory became one continued marsh, which for a series of years occasioned incredible desolation. And though these evils have since been remedied, yet by neglecting to draw off the water after inundations of the Tiber, when great heats succeed them, malignant remitting and intermitting fevers are general and fatal.†"—*Vol.* 1, *p.* 245.

This opinion of Professor Cooke receives confirmation from the fact contained in the following paragraph, taken from one of the gazettss of the day: "Dr. Chervin, of Paris, who some years ago visited all the seaports of the United States, for the purpose of investigating the nature and origin of the yellow fever, went to Gibraltar in November last, on the same errand, with some other French physicians. He wrote from that place to Paris, on the 15th December, that he and his colleagues had prosecuted their enquiries and examinations with unremitted assiduity and care, *and that he was convinced of the identity of the pestilence with the yellow fever which he had seen in America.* He denies that it is *contagious* any where."

Some objections having lately been advanced, by Dr. Ferguson, against the general doctrine, our author proceeds in the sixth chapter, to examine them. Without giving the whole of the chapter, it would be impossible for us to present the chain

*Lind on Hot Climates, p. 61. †Pringle on the Diseases of the army, p. 192.

of argument in its consistency and force; we will therefore pass over it, barely remarking that we consider the refutation complete.

In chapter 7th, going a step farther than other etiologists, the author assigns as the cause of fever, a substance which is cognizable to our senses; namely, *carbonic acid gas.*

"A dense gas arising from putrefying vegetable matter, being the cause of the disease observed in the neighbourhood of marshes; and the carbonic acid gas being the only dense gas known to arise in such circumstances, we are led to infer that the carbonic acid gas is the cause in question; and the truth of the inference is strongly supported by a comparison of the properties of the gases, of the circumstances in which their effects appear and disappear, and of the nature of these effects.

"Both these gases are denser than the atmospheric air, so as to remain at the lower part of it and to drive with the wind along the surface of the earth.

"'Dr. Dalzelle says he once checked a bilious fever by spreading twelve barrels of lime over a piece of marshy ground, from whence the exhalations which produced it were derived.'* The carbonic acid gas has a strong disposition to combine with lime; it unites with it in preference to the alkalis."—*Vol.* 1, *pp.* 283, 4.

"A wood, or thick growth of bushes, interposed between a marsh and a dwelling house, saves the inhabitants from the effects that would follow but for the interpositon. It is impossible that a gas should be unable to pass through the trees with the wind which brought it thus far. It is manifest that it is *arrested* by the wood; and the fact of the carbonic acid being *consumed* by the wood, completely accounts for the salutary effects of its interposition.

"It is a matter of common observation, that the blood drawn from persons who have been exposed to the exhalations arising from marshes, is darker than usual It is often nearly black, even in those persons who are not ill, the darkness being in proportion to the degree of exposure."—*Vol* 1. *p.* 285.

"To sum up the whole, the unknown gas is dense, and arises from putrefying vegetable matter.

The carbonic acid is a dense gas arising from putrefying vegetable matter, and the only one known to exist.

The miasmatic diseases are most prevalent in the afterpart of the summer, showing the abundance of the unknown gas.

At the same time carbonic acid gas abounds, in consequence of the profusion of dead and decaying vegetable matter.

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*Rush's Works, Vol. 4, p. 131.

The unknown gas drives with the wind.

Carbonic acid gas does also.

The unknown gas, according to Dr. Dalzelle's account, is absorbed by lime.

Carbonic acid gas has a strong disposition to combine with lime.

The unknown gas is readily absorbed by water.

Carbonic acid is readily absorbed by water.

Large fires in great number in a town, increase the effects arising from the unknown gas.

Carbonic acid gas is produced in immense quanties by them.

The unknown gas is arrested by a wood, and prevented from producing its effects beyond it.

Carbonic acid gas is consumed by a wood, and thus effectually arrested.

The unknown gas changes the blood of those exposed to its influence to a brown, and finally to a black.

Carbonic acid gas does the same.

Carbonic acid gas produces a sense of uneasiness, chilliness, sickishness, head ache, &c., a small, frequent, and irregular pulse, feverishness, &c.

The unknown gas also produces a sense of uneasiness, &c. &c.

The unknown gas extinguishes lighted candles, and destroys animal life suddenly.

Carbonic acid gas does the same."—*Vol.* 1, *pp.* 287, 288.

To all this we must be permitted to say *peut etre et peut etre non*: we see men only *as trees walking*. The facts are ingeniously arranged, and in solid phalanx appear imposing; but we do not find the labourers in distilleries, breweries, and other establishments where carbonic acid gas is perpetually liberated in large quantities, more subject to febrile disorders than any other class of the community, and must therefore withhold our assent at least for the present. However,

"The general doctrine is not implicated in the discussion respecting the *nature* of the dense gas, known by the name of marsh miasmata &c. Whatever be the nature of this gas, it renders the blood brown, and finally black, which thereby becomes less stimulant to the heart. Nor is this effect confined to this dense gas; any gas arising from putrefying vegetable matter, has the same effect of weakening the action of the heart, if prevented from flying off, or if retained so as to be respired. Dr. Beddoes says, "he never saw an instance in which a lowered atmosphere did not at the moment quicken the pulse, while it weakened the action of the heart and arteries."* But this gas,

*Darwin's Zoonomia, Vol. 2, p. 479.

being the only one dense enough to rest on the surface of the earth, is the only one actually present when the effect is produced."—*Vol.* 1, *pp.* 289, 290.

The origin of *winter epidemics* occupies the 8th chapter; and these like the epidemics of the warm seasons, the author refers to this vapour or gas. Continuing to effect the persons exposed to its operation, producing sallowness, black blood, black and green discharges from the stomach and bowels &c. it is excited into active disease by exposure to cold.

"In the course of the autumnal season, fever is often induced in persons under the influence of miasmata, by exposure to cold, sometimes during sleep in cool nights, or by neglecting to change wet clothes. If, while the population are still under the influence of miasmata, the access of cold weather be sudden, or the weather be severe, the operation of cold being general, greater numbers must necessarily suffer an attack; and this is particularly the case in winters following seasons in which miasmata abound."

"It is manifest from this statement that these winter diseases occur in persons exposed, while under the influence of miasmata, to the action of cold; and that the symptoms observed are those produced by miasmata, in connexion with more or less of those produced by cold. Wherefore they are effects of the joint operation of miasmata and cold." —*Vol.* 1, *p.* 293.

This theory is fortified by a variety of facts, but as the peculiar views of the author on this subject, have been unfolded by his own pen, in a previous number of this journal, we deem it unnecessary to enlarge upon them here.

In chapter 9, the author insists upon the *identity of the autumnal epidemic diseases.*

"From the identity of the cause of autumnal epidemics, we infer the identity of these diseases. We shall not, however, rest the doctrine on this inference alone, but consider the question at large.

"On the degree of the cold stage is founded the division of autumnal fevers into intermittents and remittents. Both these arise in the same circumstances of heat, moisture, &c. Few epidemics occur in which either is exclusively observed; one predominates, but some of the other almost universally appear also. In many cases it is difficult to say whether the fever is strictly remittent or intermittent. Remittents often become intermittents; and intermittents remittents. The change from one to the other sometimes occurs in the same person more than once. These are therefore one disease."—*Vol.* 1, *p.* 316.

"The yellow fever occurs, in hot countries, in situations, in which, in cooler countries, common remittents and intermittents would appear."

"It is a common observation that those who have had obstinate agues in England or Holland, almost constantly suffer a relapse when they arrive on the coast of Africa."—*Aol.* 1, *p.* 319.

"In the United States the yellow fever always makes its appearance in places noted for the prevalence of the ordinary autumnal fever; and in the same circumstances which produce the latter. It appears only when the cause of the ordinary grade of fever is in greater force than common. The gas which produces autumnal fever being the effect of the operation of heat on moist vegetable matter, the quantity is of course proportioned to the degree of heat where the latter abounds. In very hot summers the quantity produced must therefore be greater than common, and the grade of their effect, autumnal fever, higher. This is what we find actually occurs. In the forepart of a very hot season, the temperature is as high as it is in any part of common seasons; therefore in the forepart of such seasons the grade of autumnal fever is as high as the highest in common seasons; and, as the season advances, the grade advances also, and the highest grades appear."—*Vol.* 1, *pp.* 319, 320.

For reasons equally strong, diarrhœa, cholera and dysentery, are arranged under the same head, all belonging to the same class. This is simplifying very far, but simplicity is one of the attributes of nature. It is the province of philosophy to classify and make plain.

Of contagion, considered as a remote cause of fever:—This constitutes the matter of the tenth chapter. The author denies that fever has ever an origin in this source. On this vexed topic, it is needless for us to occupy space or spend words. American physicians, we believe, have but one opinion in relation to it. Whatever may be the disagreements of doctors elswhere, in our country we are all of one mind, that fever is the product of vegetable decomposition.

Chapter XI. *Origin of the Plague.* At one period, this disease proved to Europe the heaviest scourge with which it was ever visited, sweeping off, in some instances, one half or three fourths of the population of the places afflicted. "This," remarks the author, "incredible as it may seem, is not at all improbable if we consider the state of the country, the manners of the people, their habits of living, and the dreadful

treatment of the sick through fear of infection, with the absurd or inadequate means used for their relief." London, at present the emporium of the world, consisted, before the great fire in 1666, of "numberless small buildings, without arrangement, the streets being crooked, narrow, and filthy. The habits of the people of those days, were very different from what they now are. Erasmus, in a letter to a friend respecting England, says, 'The floors are commonly of clay, strewed with rushes, under which lies, unmolested, an ancient collection of beer, grease, fragments, bones, spittle, excrements of dogs, cats, and every thing that is nasty.'"

With such a fomites of disease in every house and such filth in the streets, acted upon and aggravated by moisture and intense heat, it is not to be wondered at that the most malignant disorders desolated the land. The following table of the author exhibits a tragic picture of the times.

"The first row of figures indicates the year; the figures opposite to the names of the months, indicates the number that died in that month in each year, in London. The last column contains the numbers that died of the plague at Dantzic, in 1709.*

	1593.	1603.	1625.	1636.	1665.	1709.
April	138	26	85	37	2	
May	167	83	224	162	43	
June	468	362	954	440	1060	319
July	2930	2999	5887	456	5667	1313
August	2880	8919	16455	1239	17036	6139
September	2200	12504	9979	3856	31569	8303
October	1260	4012	1514	2786	9444	4932
November	710	1352	256	2602	3449	1961
December	290	324	37	640	734	584

Here we see the mortality keep pace with the increase and decrease of the heat of the season, with just such occasional variations as we know occur in the temperature of different summers; just such as have occurred in our epidemics, connected with known variations of temperature."—*Vol.* 1, *p.* 405.

From all this we may learn that the plague has its origin, in common with the autumnal epidemics of our country, in the decomposition of vegetable matter. It appears, in all the cities of lower Egypt, where its annual ravages are felt, under the

*Russel on the Plague, p. 276.

same circumstances which give rise to the fevers of our climate. The annual overflow of the Nile, inundating a country of the most luxuriant vegetation, where "from the end of February, the heat, even at nine o'clock in the morning, is hardly supportable by an European," is a most prolific source.

"This is confirmed by observing that the effects produced in both cases are alike; or, that the symptoms of plague and of autumnal remittents in every part of the earth are the same, *there being no essential difference between them.*"—*Vol.* 1, *p.* 421.

"There is in the description of this epidemic, the most pointed and marked resemblance to our epidemic fevers, with the single exception that buboes and carbuncles are rare in the latter. It is difficult, and almost useless to particularize. Considered as a description of our fevers, we assent to its truth and excellence, sentence by sentence. It has been read to several physicians, all of whom considered it as a description of the epidemic they had been familiar with, and as a very good one."*—*Vol.* 1, *p.* 436.

The origin of *Typhus Fever* is considered in the 12th Chapter. It is the opinion of the author, who has fenced himself about with his usual array of facts, that it is the product of the great laboratory of pestilence which generates the preceding diseases. We will extract a few passages referring to the work itself for abundant proof.

"The physicians of Liverpool, in their report to the corporation, allege the necessity of draining certain streets, because to them, "the low fevers, which *in the autumnal months especially* infest these streets, are principally to be imputed."

Dr. Ferriar, speaking of the health of Manchester, says, "In the summer and autumn of 1794, the usual epidemic fever (a mild typhus) became very prevalent among the poor in some quarters of the town, particularly after a *bilious colic* had raged among all ranks of people. To the ordinary causes of fever were now added the influence of *a burning summer*, succeeded by *very wet but yet warm weather.*"†—*Vol.* 1, *p.* 459.

"If therefore we consider the weight of evidence arising from the consideration of typhus fever and fevers acknowledged to be miasmatic appearing in the same situations and seasons, the same kind of weather being favourable to the prevalence of both, of the symptoms being

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*Those few sentences in which buboes and carbuncles are mentioned were not read.

†Ferriar's Medical Histories, p. 231.

the same, of their being convertible into one another, of epidemics manifestly miasmatic being called typhus by a distinguished nosologist, and by others, of the distinction made between the different cases of the same epidemic whereby every one may be divided into two, of the admission of a common origin of typhus and miasmatic fevers by English physicians of the most extensive opportunities for observation, there cannot remain a rational doubt of the truth of the doctrine, that typhus fever is produced by miasmata."—*Vol.* 1, *p.* 522.

The 13th Chapter is taken up with the proof of the "Identity of autumnal remittents, typhus fever, and plague;" and the 14th, with the "Unity of fever." "There is therefore but one fever." This is the conclusion of the whole matter.

The 15th Chapter, showing that a general epidemic constitution of the atmosphere is not the cause of fever, is uncommonly able and interesting. It takes a general survey of the epidemic disorders which at different times have prevailed in our country, and shows that wherever they have appeared, whether in Virginia or Mississippi, on mountains or in vallies, they have been attended by their invariable concomitants,—humidity, and a high temperature of the atmosphere.

"It is evident, therefore, that in dry seasons health is general, and in wet seasons sickness is general; that the sickly spots in dry times are near streams or marshes; and that when one part of the country is dry and another wet in the same seasons, the dry part is healthy except about streams and marshes, and the wet part sickly."—*Vol.* 1, *p.* 539.

Some general remarks on the origin of epidemics follow in Chapter 16th. Additional proof is furnished, that all fevers are the product of miasmata. The case of the old Baily is thus explained. This has been considered an *experimentum crucis* of the truth of contagion.

"The disease which caused the death of a number of persons in London, at the sessions at the Old Bailey in May 1750, has been very confidently attributed to contagion; but the occurrences were utterly inconsistent with the production of the disease by such a cause. Even the variolous contagion, it is well ascertained, affects only those who are very near to the sick; but the persons who suffered on this occasion were far beyond the reach of any known contagion, viz. about twenty five feet from persons who were supposed to have carried the contagion into court. Moreover, most of the persons who were attacked were at the farthest end of the court-room, while those imme-

diately around the prisoners are not represented as having been affected. Lastly, the singular circumstance of almost every one who was attacked being on the left side of the room, and in elevated seats, points to some other cause than one which, if it exist, must extend alike to all around. It is plain enough, therefore, that the cause of this disease could not have been contagion; what it was, is as plain from the statement of Bancroft.*

"The court was excessively crowded to hear the trial of a captain in the navy for killing another in a duel. The trial was very long, and the house was so close that the air became in a short time exceedingly offensive; and although the weather was cold, a cold piercing east wind then blowing, the room was very warm. A window was therefore opened on the left side of the house, and a current of cold air forcibly rushed in, and, the window being much higher than the heads of the people on the floor, passed over them to those who were more elevated, and therefore exposed to its action. Hence the number affected on the left side of the house. Such exposure to cold air, when the person is much heated, has often produced violent fever."—*Vol.* 1, *p.* 552.

The 17th is a short chapter of definitions, and is the last in the first volume. Some additional remote causes are enumerated.

We have thus given a cursory and imperfect view of the matter of the first volume. We are conscious that we have not done justice either to the author or our readers. The connected and strong chain of argument which extends throughout the volume, cannot be perceived in such an analysis as we have given. But our review may occasion some to look into the work who have not read it; and if such should be the result of our labours, we will consider ourselves amply repaid.

L. P. Y.

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*Bancroft on Fevers, p. 443.

[TO BE CONTINUED]

MISCELLANEOUS INTELLIGENCE.

THE BANDAGE.

In consequence of the public attention which has lately been drawn to this agent, more particularly as a powerful resource in the treatment of surgical diseases, Professor Dudley has received several communications from practitioners in different parts of the country, detailing cases of its beneficial application, and corroborating his views of its important utilities. These have been placed at our disposition, and we take the liberty of making some extracts from them.

Case 1.—White Swelling &c. *By Dr. Archibald Debow, of Hartsville Tennessee.*—"In the month of August last, I was requested to see a negro girl, about ten years of age, who had, for the last four or five years been labouring under a white swelling of the leg, which had hitherto baffled all attempts to cure it; the case being considered by two or more physicians as hopeless, "unless the constitution should be strong enough to overcome the force of the disease." When the girl was placed under my care, there were three large sores on the leg, from which a most offensive matter was constantly discharging, together with scales of bone (the tibia) very frequently, which at once proved to me that the bone was materially affected. The leg was swelled to twice its proper size, and every circumstance, both as regarded the situation of the leg and the constitution of the patient, went conclusively to show, that unless something was done soon, she must die as a consequence. Of the origin of the disease I could get no definite information; a slight abrasion of the skin being the first symptom that had been noticed, which inflamed, suppurated, and continued to grow worse constantly.

"This being the first case that I had an opportunity of testing the efficacy of the bandage upon, I resolved to try it, though prejudice was somewhat excited against it in consequence of a fear of mortification, by the owner of the girl. The bandage, however, was applied, with considerable firmness, from the ends of the toes to above the knee. She

was put upon an alterative course of mercury; and rest, with a diet of milk and bread or mush, was enjoined. For the first two or three weeks no material alteration was discoverable, other than a subsidence of the swelling, and very many scales of bone coming away. I now thought of trying counter-irritation, by laying open the leg; which I did to the extent of two and a half or three inches, when I extracted a portion of bone about three and a half inches in length, which made an opening of nearly the same extent into the hollow of the tibia. I now cleansed the wound with warm water; placed a piece of sponge in it; drew the edges together with adhesive strips, and put on the bandage as firmly as I could without giving too much pain. This dressing remained on about forty-eight hours. Upon taking it off, I found the sponge had acted on the lips of the wound in a manner similar to what it did in your case of fungus cerebri, before you put the muslin between the sponge and brain. The edges were healthy in appearance, and bled a little. From this time I did nothing more than draw the edges together with adhesive strips, and kept the bandage firmly applied. It now began to suppurate freely and healthily: its fœtor ceased, good pus was discharged, and about two months from the time I commenced treating it, the dressings were all taken off, and the leg was well. The constitutional derangement was entirely relieved, and she has since enjoyed good health.

"With the bandage I have successfuliy treated several cases of paronychia; and in one case, in which matter was formed to a considerable extent in the thumb, it was absorbed entirely, leaving the thumb without even the loss of the nail, which had every appearance of coming off. In cases of common sore leg from local violence, which had for weeks resisted the usual modes of treatment, I have cured in a few days with the bandage. In cases of sprains of the ankle and of the wrist, and in all bruises, I have invariably found it to act as a charm: nor do I entertain a doubt but in all the cases in which you so strongly recommend its use, it will be found equally efficacious."

March, 1829.

Case II.—Gun-shot wound. *By William C. Turner, M. D. of Richmond, Kentucky.*—"On the 29th of December last, Mr. M. S. having loaded his rifle-gun for the purpose of sporting, was crossing a yard stile; and having ascended to the topmost step, placed the breech of the gun upon it: from this it slipped off, and in falling struck the

cock against the edge of the stile, which fired the piece, discharging the ball in the axilla of the right side. He fell and bled profusely until syncope ensued, when the hæmorrhage ceased, after the loss, as was supposed, of half a gallon of blood. My co-partner Dr. Venable and myself were sent for. On arriving at the house of his relative, where the accident had happened, we found the young man breathing with considerable difficulty and anxiety; his colour pale; pulseless in the wrist of the wounded arm, which had lost all power of voluntary motion; and but feeble action could be felt in the artery of the other, although its pulsations were distinct. These symptoms led to the fear that the lungs were injured, and from the solicitations of his friends to know whether or not the wound was necessarily fatal, a probe was introduced, and from the result of the examination it was concluded, that the ball had passed through the axillary glands, dividing the brachial plexus and wounding the axillary artery; continuing its course through the cellular substance and muscles, it struck the clavicle about one inch from its outer extremity. The bone here obstructed the ball, and probably turned its course, for it could not be farther followed with the probe, or felt externally; consequently no efforts were made to extract it. It was hoped that the lungs were not injured, and that if the recurrence of hæmorrhage could be prevented when reaction came on, the only danger would be from mortification and secondary hæmorrhage.

"Before any reaction was manifested a roller was applied from the hand to the shoulder; compresses were placed upon the front part of the shoulder corresponding with the course of the wound, and bound firmly on. By repeated turns of the bandage over the shoulder and under the axilla of the opposite side, crossing behind and in front of the chest, the whole tract of the ball was under its direct influence, save the point of entrance and inferior portion, where pressure was prevented by the cavity formed by the insertion of the pectoralis major and latissimus dorsi.

"The young man was dressed and carried to his father's, about a mile distant, that evening; taking care to keep him in a recumbent posture, for any attempt to sit erect was for several days followed by faintness. The difficulty of breathing and dull head-ach continued through the night. The next day, while feeling through the bandage for the pulse in the radial artery, I thought I could discover it, and on removing the bandage to the place, it could be felt pulsating feebly though distinctly. On the third day from the accident, anxious that the bandage

should have its best possible effect, the whole of it was removed, and reapplied. There was no appearance of suppuration at the entrance of the ball, and the chief complaint was of a numbness in the fingers and arm. The bandage was continued on for some length of time, with a view chiefly of keeping the shoulder quiescent, until the newly formed adhesions should acquire sufficient strength not to be ruptured by extension, for newly formed parts are easily lacerated. He has but partially recovered the use of his arm and fingers, although there is a gradual improvement in this particular.

"That the axillary artery was divided in this case there can hardly be a doubt, for the same quantity of blood would not have been discharged from a wound of any of the small vessels of that part, even if made by a sharp cutting instrument, much less by a gun shot. That there was a reunion of the artery is as certain as that it was divided, for anastomosis would not have been completed in so short a time."

March 1st, 1829.

Case III.—Mortification &c. *By Stephens R. Beatty, M. D. of Maryland.*—"Not many weeks since, while treating a case of ulcerated leg, in which forty-seven ulcerated openings, situated between the toes and knee had been healed in a few days by the use of the bandage, the patient, merely because the bandage produced some little uneasiness where the tendons of the flexor muscles of the foot pass the ankle, removed it from the toes to the ankle, leaving the portion on the leg uninterrupted. He reapplied the bandage much looser on the foot than it was on the leg, and irregularly. On visiting him a few days afterwards for the purpose of dressing, I was surprised to find the foot of an unusual size. When the bandage was removed, a mortification of several square inches was discovered on the instep, and the skin of the whole foot rendered tense by œdematous swelling and inflammation. The bandage may be blamed for this! Admitted. After elevating the leg, and reducing the swelling of the foot by pressure with the hands, the bandage was again applied firmly to the foot, and loose to the leg. On removing it 48 hours afterwards, the inflammation and swelling of the foot was gone, the gangrene stopped, and healthy granulations were shooting up under the slough. In a few weeks it was perfectly cicatrized. This is an instance of an injury being *cured* by a different application of its existing cause." *February 17th*, 1829.

Case IV.—Fracture of the Femur. *By Dr. S. Benthall, of Shepherdsville, Kentucky.*—"On the 3d of September, 1828, I was requested to visit John Newkirk, aged 21 years, of this place, who, in falling from the roof of a house, fractured his thigh about the middle. The bandage as recommended by professor Dudley, was applied, beginning at the toes and enveloping the whole limb; two turns of the bandage were carried around the body at the hips, and then several turns back around the thigh, a little lower than the fracture. Two pieces of paste-board were then applied, the better to support the limb. At the time of applying the bandage there was but little swelling, as it was only two hours after the accident when the application was made. About 12 o'clock on the night of the 11th I was called to see my patient: he complained of pain in the broken limb, the belly and back, and seemed in the most excruciating suffering. As there was considerable fever with a quick, strong pulse and a plethoric habit, I bled him xxxij oz. and gave him ol. ricini with 50 drops tinct. opii. There was considerable swelling and coldness of the limb at this time, but the other unpleasant symptoms yielded in about an hour, and by a spare diet and open bowels, fever was kept off; the swelling of the limb gradually lessened, insomuch that it became necessary to reapply the bandage on the 10th day; and by the 30th the patient was able to walk; and is now as well as he ever was. Only three applications of the bandage were necessary." *March 25th* 1829.

The effects of the bandage, in speedily relieving almost every variety of swelling and ulceration, and producing rapid cure in cases which any other mode of treatment, either fails to relieve or does it in a very tardy manner, are not to be learned at this day by the great body of the junior physicians of the West. A case occurred in my practice, however, in the course of the last winter that deserves recording among other facts on the subject. A medical gentleman Mr. B. desiring me to visit him, I found him very much reduced in flesh, with an excessively irritable stomach, and great depression of spirits. His evacuations were procured with difficulty and were quite black. He had also an extraordinary ulceration and enlargement of the glans penis, attended by pain so excruciating that he was continually writhing with pain and frequently wept. His complaints were so great and continual, and the ulcerations were so deep in the substance of the part, that I was at a loss to name the disease; and every mode of treatment, I could devise, totally

failed in relieving the swelling or pain. He was after several days' trial of various remedies, advised to use the bandage. This however he did not attend to. In the mean time, he was every day taking cathartic medicines, and discharging black matter; and in four or five days was setting up by the fire. The ulceration, pain and swelling of the glans penis however continued unabated; and finding him one morning writhing in his chair and actually weeping from the severity of his sufferings, I told him I was surprised he did not use the bandage, and mentioned instances in which I had known continued pressure, even made by merely rubbing with the hand, relieve painful swellings. I concluded by urging him, as I had repeatedly before done, to send for Professor Dudley. The next time I visited him, he was sitting at ease and perfectly cheerful. Surprised at this, I inquired how it happened. He told me he had sent for Professor Dudley, who had applied the bandage to the part and that in ten minutes he was easy. In a few days he rode home in a carriage, a number of miles. C.

Epilepsy *cured by an operation with the Trephine.* Extract of a letter from Samuel A. Cartwright M. D. of Natchez, to Professor Dudley.—"About two weeks since, I was called in consultation to see a respectable cotton planter of this vicinity; and as his case was somewhat peculiar, and as your essay, in the first number of the Transylvania Medical Journal, considerably influenccd us in the treatment, I will briefly relate it. The gentleman received a blow on the head with a cane which cut through the integuments down to the parietal bone, near its junction with the frontal bone on one side. After a few moments of insensibility he recovered from the shock and rode several miles to his plantation. A strong epileptic fit occurred in about twelve hours after the accident. It went off and recurred in the course of three or four hours. Dr. Elliott, his physician, bled and purged him freely with some benefit, only temporary however. The fits at length began to occur more frequently and to continue longer. On the third day, I was called to see him. The intervals between the fits had now become very short and he continued stupid the while, whereas before he was sensible and could converse during the intermissions. His brother had died some time before with similar convulsions, not however from the same ostensive cause. Both had been occasionally intemperate.

"The wound was dilated and the cranium particularly examined, but

no fracture existed. Dr. Elliott and myself, however, concluded to trepan him. The Doctor operated—A large coagulum of blood was found on the dura mater, which being removed the patient became sensible, got up and walked from the table to the bed. He has had no return of the epileptic fits since, and is now convalescent."

November 3*d* 1828.

PARALYSIS *cured by Lightning.*—A vessel, while pursuing her course across the Atlantic, was struck several times with lightning, and the men were strongly electrified. Among the passengers was a man who had been paralytic of both lower extremities for more than three years. He was in bed when the electric discharge took place, and his shipmates were not a little astonished to see him jump out of his cot and march upon deck, where he continued to walk about as brisk as any of them. The cure was not temporary; for he has enjoyed the use of his limbs ever since. We have lately seen some remarkable effects produced by strong shocks of electricity thrown along the nerves leading to paralyzed muscles:—also in torpid states of the liver, when the galvanic fluid was thrown through that organ.—*Medico-Chirurgical Review, October*, 1828.

Remarkable Case of NEURALGIA *By Mr. Dod.*—Among the numerous suggestions that have been transmitted to us respecting the distressing case of neuralgia, published in the present Number of this Journal, there is a case related by Mr. Dod, late of Bath, and now of Caroline Street, Bedford Square, which we deem it a duty to communicate to the public, and that in the words of the communicant.

"The distressing case of neuralgia inserted for consultation in a recent fasciculus of the Medico-Chirurgical Review, induces me to communicate some particulars of a most inveterate case of this complaint, which occurred in the person of an officer, who had been most dreadfully lacerated by a tiger in the East Indies.

"The patient was Lieutenant Saunders, (now Captain) of the East India company's service, about 38 or 40 years of age. This gentleman after having resided in India about eight or nine years, was recommended to return to England on account of a liver complaint, the sequela of a *jungle fever* he caught during some military operations in which he had been engaged in the country. After a short residence in England, his health being restored, he again returned to India. Upon

his return, and during some part of the Poona war, in which he was conspicuously engaged, he one day incautiously approached a tiger, which sprang at him, seized him, and lacerated him in a most dreadful manner, destroying with its claws almost all the pectoral muscle, and other motors of the humerus of the left arm. The tiger having left him, he lay in this mangled condition for a considerable time before assistance could be procured, during which interval he bled profusely. He recovered from his wounds; but there supervened one of the most distressing neuralgic affections of the body that can well be imagined. He became afflicted with violent head-achs, which increased to such a degree of severity, as sometimes to deprive him of reason. Coupled with these distressing head-achs, there was great pain in the lacerated parts, with subsultus of the muscles and tendons, which frequently extended to those of the body, giving to the spectator the idea or appearance of its being an epileptic fit with which the patient had been seized. His digestive organs were much disordered, and his food in general disagreed with him. When his stomach was most out of order, all his sufferings were exasperated. These distressing complaints continued to increase in severity from the time of the accident until I saw him, about two years ago, at Truro, to the neighbourhood of which place he had retired to pass the remainder of his miserable life, after having sought relief from some of the most eminent of the faculty in London and other parts of this country. When I saw Mr. S. he exhibited a most distressing spectacle;—His body was emaciated—his countenance was pale—his lips blanched—and his features shrunk. The functions of his digestive organs were much disordered. His extremities were, in general, cold; and his pulse, which was small, beat about 120 strokes in a minute. When paying my second visit to him, I had an opportunity of witnessing one of those nervous seizures above mentinned, and of noting its progress. The subsultus commenced in the muscles of the lacerated arm; then extended to the eyes and muscles of the face; and, lastly, over the whole body. Some of the muscles seemed in a spastic state, whilst others were in a state of subsultus. He did not struggle! In this condition he remained about ten minutes, and then gradually recovered. On the preceding night, he had suffered severe pain in the lacerated parts, with violent head ach; the latter symptom continued after the seizure. Although the subsultus frequently took place in the lacerated parts, yet it did not always extend to the body.

"I prescribed the following medicines, which had proved successful in a severe case of neuralgia a few years before.

"R. Extract. colocynth. comp.—extr. jalap.—extr. alois spicat. *aa* 2 scruples. Mass. pilul. hydrarg. 1 scruple. Contunde et divide in pillulas xxx æquales.

"Capiat decoct. sarsapar. simp. lbj. quotidie.

"Balneum calidum 3tio vel 2ndo die.

"I ordered him to take one, two, or three of the pills *every night going to bed*, so as to produce, the next day, two or three evacuations from the the bowels. The sarsaparilla he took regularly every day, but the warm bath was not attended to so regularly as I could have wished.

"These were the *only* medicines and means made use of, until my patient perfectly recovered, which he did in *twelve weeks* from the time I first saw him. He embarked the following spring for India, and has continued in good health up to the last accounts I heard of him.

[*Medico-Chirurgical Review, Oct.* 1828.

Case of Epilepsy *of twelve years duration cured by Valerian.* By John Gairdner, M. D. F. R. C. *Surgeons, Edinburgh.*—In the beginning of January 1814, I was desired to see a married woman, about 40 years of age, and mother of several children, on account of epilepsy, to which she had been liable for more than twelve years. The paroxysms were generally preceded by a sensation of *globus* in the throat. They commenced with a scream, which was immediately followed by total insensibility, violent agitation of the arms and legs, and contortions of the features. These convulsive efforts gradually gave way to sopor, and a considerable degree of drowsiness lasted for the rest of the day. She had no remembrance after these fits of any thing that had occurred in the course of them. During the whole of the long period that she had been subject to them, there was never any regularity observed in the time of their recurrence, but the intervals had been progressively decreasing. At first she was often exempt from them for several months together, but at the time when I was called she was visited by this very distressing disease twice or three times a-week, so that she had been forced to get a servant to assist her in her domestic duties, which she had been formerly able to accomplish without such aid. She sometimes had two or three attacks in twenty-four hours; but when this happened, the second and third attacks were less severe, and did not produce total insensibility.

I had very slight expectation of being able to do this poor woman any good, more especially as she had already taken without benefit a variety of remedies by the advice of several highly respectable practitioners of this place. But I was unwilling to pronounce the case desperate, and it flashed upon my memory, that I had somewhere read of the successful treatment of a similar case by the powder of valerian. I accordingly recommended to her to take a scruple of this powder night and morning. Two days after she had begun the use of it she had a pretty severe fit. She continued taking the powder four or five days, till she had taken in all twelve doses of one scruple each, and then, finding herself free from the fits, she omitted the medicine without consulting me. She had a second fit, but much slighter, a fortnight after the first, and a third slighter than the second at the end of three weeks more. On both of these occasions she reverted to the use of the valerian. The memorandum of the case, which I have preserved in my note-book, is dated in May 1814, four months after I first saw her. She had at that time taken in all only thirty-six powders, and had been perfectly well from the time of the third attack. She has ever since continued perfectly free from the epilepsy, though she has at different times been induced to take the valerian, warned by such sensations as used formerly to indicate the approach of the fits. Any thing which is calculated to frighten or to startle her brings on these sensations, which are also superinduced by grief and other depressing passions. In the year 1820 she lost her husband, and the fatigue and anxiety she suffered during his last illness brought on many of the old feelings, together with some fits of convulsive agitation, followed by drowsiness, but not preceded by a scream, nor attended with insensibility. They were of short duration, and were speedily banished by that remedy which has proved to her so great a blessing. They must have proceeded from the same cause which produced her former fits; and yet they cannot strictly be denominated *epileptic*, destitute as they were of the essential characters of epilepsy.

I visited this patient within these few days,* and found her in excellent health, and in full possession both of her bodily and mental powers. She has not taken the valerian for more than six months, having all that time been remarkably free from those ominous sensations which formerly induced her to resort to it.

*This account was written in 1826.

I conceive that I am fully entitled to impute the cure of this disease to the remedy employed:—*First*, because of the great number of years that the patient had been liable to epilepsy, and the progressive increase of the frequency and severity of the fits during that time,—circumstances which destroy all probability that the cure should have arisen from any spontaneous change in the constitution of the patient: *Secondly*, because after the administration of the medicine only three fits occurred, and these at intervals progressively increasing; and, *thirdly*, because the use of the same remedy has always removed the disposition to those imperfectly formed fits, to which she had been occasionally liable since the cessation of epilepsy.

I have not ventured to occupy the time of the society with this narrative, on account of any pretensions to originality in the practice adopted, for this I have already disclaimed; nor have I done so on account of any sanguine hopes which I am disposed to entertain of the universal efficacy of valerian in a disease so various in its character and causes as that with which my patient was affected; for I never indulged expectations so contrary to all analogy, and if I had, my own experience since the date of this cure has afforded matter quite sufficient to dispel the illusion. But I bring the case forward, because I think it probable that the remedy which proved successful may deserve to be employed in epilepsy more extensively than it is at present; and because the history of the symptoms will serve in some degree to point out in what instances it may be expected to be useful. On this latter point I have only to remark, that I should be disposed to make trial of valerian in all cases of epilepsy occurring in individuals of the nervous temperament, and more especially in females who have been often troubled with globus and other hysterical symptoms.

Some of my professional friends to whom I have repeated the particulars of this case, are disposed to refuse the title of epilepsy to the disease, and to rank it as one of the varieties of hysteria. The question is purely one of nomenclature. That it originated in the hysterical temperament there can be no doubt; that the symptoms which attended the paroxysms were those of epilepsy is equally indubitable. And I apprehend that it is a principle of nosological classification pretty generally admitted, that diseases are to be arranged according to their characteristic symptoms, and not according to their presumed causes. But, however denominated, it was a disease as destructive of the comfort and usefulness of the individual as if it had proceeded from the usual causes of epilepsy.—*Edinburgh Med. and Surg. Jour. Oct.* 1828.

Effects of Ardent Spirits.—Mr. Dewar, Surgeon, Dunfermline, has given the history of two cases of poisoning with the Carbonate of Potash, one of them occurring in a woman who had long been in habits of intemperance, and whose body was examined after death.

"The first thing which attracted our notice was the enormous size of the liver. It descended to the crest of the ilium on the right side, and shelved forward so as to conceal, under its thin edge, a great portion of the intestines. It also descended into the hypogastric region to within (as we ascertained by measurement) two inches and a-half of the pubis. The only deviation from its healthy structure which we could perceive was, that it was somewhat firmer in its texture than natural.

"Whether the prodigious increase in the size of the liver ought to be attributed to her extraordinary indulgence in ardent spirits, I will not pretend to decide; but I may mention, that, for a very long period of time, she had been addicted to the use of spirits, and that, especially for nine years before her death, her excesses were very frequent, and the quantities she drank enormous. On many occasions she was constantly intoxicated for weeks, once for six weeks without intermission, during which time she drank every day not less than half a gallon (4lb.) of whisky, and on one of these days 6lb.; on one occasion she was constantly drunk for the almost incredible period of fifteen weeks; and during the whole of that time she consumed not less than half a gallon every day. During the first five of these fifteen weeks she was not undressed, nor during the last six. These facts may be depended on."—*Ibid.*

Cold Injection for Anatomical Preparation.—If a mixture of varnish and vermillion has a small quantity of water mixed with it, it soon sets and becomes hard. This affords an excellent composition for anatomical injection, being very beautiful and very penetrating, (so much so, that it frequently returns by the veins,) and requiring no heat to be applied to the subject. The writer of this article frequently had, in the course of his medical education, the office of preparing this injection, of which he has, however, unfortunately forgot the proportions, and the particular nature of the varnish. It was, he thinks, a spirit varnish; the water was not mixed until the instant the injection was wanted, when it was well worked up with the syringe, and immediately thrown in; in the course of a night it would have set beautifully.—*Brande's quart. Jour. in New York Med. and Phys. Jour.*

Transylvania University—Medical Department.

LIST OF GRADUATES, 1829.

At a public commencement held in the Chapel of the University, on Tuesday the 10th of March 1829, the degree of Doctor of Medicine was conferred on the following gentlemen, alumni of the School, who had undergone satisfactory examinations before the Medical Faculty, Trustees and President, and written Theses on the subjects annexed to their names.

I. FROM KENTUCKY.

1. Robert Stuart Apperson, of Richmond, *on Sarcocele.*
2. John Charlton Beatty, of Mason County, *on Paracyesis Abortus.*
3. Richard William Ferguson, of Louisville, *on the Physiology and Pathology of the Teeth.*
4. Charles Hay, of Fayette County, *on the Anatomy and Physiology of the Stomach.*
5. Henry Hopson, of Christian County, *on Menstruation.*
6. John Terrel Lewis, of Lexington, *on Milk-Sickness.*
7. Preston Lindsay, of Scott County, *on the identity of Summer, Fall, and Winter Epidemics.*
8. Thomas Jefferson Moore, of Bowlingreen, *on Erysipelas.*
9. Alexander Hamilton Peck, of Lexington, *on Phthisis Pulmonalis.*
10. James Ritchie, of Lexington, *on Hydrocele.*
11. Thomas Steevenson, of Meade County, *on Syphilitic and Pseudo-syphilitic affections.*
12. Robert James Waggener, of Columbia, *on the Functions of the large Intestines.*
13. Henry Francis Washington, of Russelville, *on Dyspepsia.*
14. Walter Carr Winter, of Lexington, *on Ophthalmia purulenta.*

II. MISSISSIPPI.

15. Charles Walton Harris, of Pike County, *on Lacteal and Venous Absorption.*
16. Charles Shaw, of Jefferson County, *on the Nature and Treatment of Syphilis.*
17. Solomon Tracy, of Meadville, *on Uterine Hæmorrhage.*
18. William Walworth Usher, of Wilkinson County, *on Malignant, Congestive, Bilious Fever.*
19. William Kinne Wilson, of Adams County, *on Dropsy.*
20. Ellis Pusey Passmore, of Meadville, *on Uterine Hæmorrhage.*

III. ALABAMA.

21. William Kelsay Adams, of Limestone County, *on the Medical Topography and Endemic Fever of Limestone county, Alabama.*
22. Richard Tucker Harper, of Lawrence County, *on the Analogy between the Liver and the Brain.*
23. Jacob Chancy Jordan, of Linden, *on the Operation of Lithotomy.*
24. Sydney Smith Prince, of La-Grange, *on the Medical Topography and Endemic Fever of Franklin county, Alabama.*
25. William Williams Wilson, of Monroe County, *on Sleep.*

IV. TENNESSEE.

26. Erasmus Darwin Fenner, of Jackson, *on Uterine Hæmorrhage in Pregnancy.*
27. James McKinney Gray, of Sumner County, *on Opium.*
28. John McCall, of Smith County, *on Purgative Medicines.*
29. Henry Holmes Treadway, of Murfreesboro', *on Syphilis.*

V. SOUTH CAROLINA.

30. Archibald Ingram Barron, of York District, *on Syphilis.*
31. Thomas Latta Dunlap, of Lancasterville, *on Hernia of the Abdominal Rings.*
32. Gilbert Tennent, of Abbeville District, *on the Medical Topography of Abbeville District, S. C. with Observations on Bilious Fever as it occurred there in the summer of* 1828.
33. Franklin Williams, of Lawrens District, *on Diet.*

VI. VIRGINIA.

34. William Bomar, of Halifax County, *on Hydrocele.*
35. Peter Leath Penn, of Patrick County, *on Curved Spine.*

VII. LOUISIANA.

36. Owen Connelly Blount, of East Baton-Rouge, *on the Teeth.*
37. William Henry Lyne, of East Feliciana, *on Bilious Fever.*

VIII. MICHIGAN.

38. Rice McCoy, of Carey's Station, *on the Actual Cautery.*
39. Josephus McCoy, of Carey's Station, *on the Pathology and Medical Treatment of Calculous Affections.*

IX. OHIO.

40. Rezin Thompson, of Harrison County, *on Digestion.*

C. W. SHORT, M. D. Dean.

OBITUARY.

DIED in this place on the 21st of February 1829, Mr. William R. Smith of Henry county, Kentucky, Student of medicine. Mr. Smith had attended two courses of lectures in this Institution, and gave promising assurances, by his devoted attention to study, and his highly respectable attainments, of becoming an ornament to the profession.

—— At Maysville, Kentucky, on the 10th of March, Henry William Coburn, M. D. of that place. Dr. Coburn was an eleve of the Transylvania School, in which he graduated in the spring of 1828. Feeble health, the consequence of predisposition to phthisis, had afflicted him before he completed his studies, and prevented him from entering upon the practical duties of his profession, when his career was arrested by the hand of death.

"It seldom falls to our lot to record the transition of a fellow mortal to a world of spirits, who has excited so much feeling sympathy for his death, and who appears to be a more general bereavement to our society, for one so young, than the subject of this short notice. The deceased was a young gentleman of much promise—of a lively and cheerful disposition, and of many noble and excellent qualities of the heart and mind, which fondly endeared him to an extensive circle of friends and acquaintances. He had but just entered on the stage of life; with all the gladsome anticipations of a youthful heart, to commence the active duties of his profession, when death, the destroyer came, and snatched him from among us, upon the very threshhold of his noble intentions."—*Maysville Eagle, March* 18, 1829.

——At Hopkinsville, Kentucky, on the 16th of March, Dr. Daniel Preston, aged about 60 years. Dr. Preston was born in the State of Maryland whence he emigrated to Kentucky at an early period, and took part in the military operations of the day against our Indian enemies, having served in a professional capacity, on one or more campaigns upon the North Western frontier. On his return he settled himself for a short time as a practitioner in Lexington, but soon left this and became located permanently in the adjoining county of Woodford, where, for many years, he was esteemed a safe, judicious and successful practitioner. About fifteen years since he relinquished the farther practice of his profession, and removed to the western part

of the state of Kentucky, where he had since devoted himself to agricultural and mercantile pursuits. As a man he was charitable, honest and kind. As a citizen, he was public-spirited, industrious and useful. As a friend he was devoted, generous and sincere. S.

— In this city, in the month of February, Doctor Truman Bishop, Physician and Minister in the Methodist Espiscopal Church. Dr. Bishop was a man of gentle manners, exemplary piety, and impressive, unaffected, pulpit eloquence. He had not practised physic to much extent for several years; but at the time of his death, was vice president of the First District Medical Society of Ohio, and a member of its Board of Censors.—*Western Jour. Med. and Phys. Sciences, Feb.* 1829.

—— At Newport, Ky. in September last, Dr. Thomas Hinde, at the advanced age of 92 years.—*Ibid.*

—— At his seat of *Chaumiere des Prairies* in Jessamine county, Kentucky, on the 9th of March 1829. David Meade Esqr. in the 83d year of his age.

Although the annunciation of the death of an individual unconnected with the profession may seem irrelevant to the matter of a medical Journal, yet as a practical cultivator of Botany Mr. Meade occupied a pre-eminent station in a country where devotees to this delightful science, either in or out of the medical profession, are so extremely rare. At his beautiful country seat he had made a greater collection of the indigenous vegetable productions of the western country, than is to be found in any other part of it; for in the embellishment of this spot, which had been the labour of more than 30 years, he justly preferred the hardy natives of the soil, to the sickly and tender products of exotic regions. The collection is especially rich in native trees and shrubs; and a catalogue of these, with many annuals of the more showy kind which he had introduced, would form no incomplete epitome of a Western Flora.

Of the correctness of Mr. Meade's taste as a landscape gardener; of his extended hospitality and gentlemanly deportment, all have borne witness who have ever visited Chaumiere; while to his more immediate friends and acquaintances he was endeared by many excellent virtues. But a few months before his own decease he had suffered the deepest affliction in the loss of a beloved and venerated consort, with whom he had lived in the tenderest bonds of affection for more than fifty years. S.

Meteorological Journal, from January to March, 1829. By THOMAS J. MATTHEWS, A. M. Morrison Professor of Mathematics and Natural Philosophy, in Transylvania University.

FOR JANUARY.

Date.	Thermometer. Sunr	3 P. M.	Suns t.	Mean of Barometer.	Prevailing Winds.	General Remarks.
1	58°	67°	60°	28,800	w. & sw.	Cloudy, clear.
2	41	33	28	29,128	nw.	Cloudy, rain.
3	20	39	34	29,077	w. & sw.	Cloudy.
4	30	50	38	29,215	south.	Clear.
5	32	51	50	29,207	s. & se.	Clear.
6	47	60	52	29,342	sw.	Cloudy.
7	55	62	55	29.293	sw. & ne.	Cloudy, rain.
8	50	50	34	29,060	north.	Rain, cloudy.
9	23	23	19	29,277	nw.	Snow.
10	7	19	14	29,547	nw.	Cloudy.
11	10	26	24	29,577	nw.	Cloudy.
12	24	31	32	29,363	e. & se.	Snow.
13	34	37	38	29,185	east.	Rain.
14	45	42	38	29,000	e. & nw.	Rain, cloudy.
15	36	44	37	28,908	sw.	Cloudy.
16	30	39	30	28,925	sw. & nw.	Variable, cloudy.
17	27	34	30	28,938	nw.	Cloudy.
18	27	36	33	29,052	se.	Clear.
19	26	44	40	29,115	se.	Clear.
20	38	44	34	29,143	sw. & w.	Rain, clear.
21	28	40	32	29,260	sw.	Clear.
22	30	42	34	29,159	sw.	Clear.
23	28	42	41	29,002	calm.	Clear.
24	27	43	42	28,973	east.	Cloudy, clear.
25	44	48	47	28,776	sw.	Cloudy.
26	47	50	33	28,648	sw.	Cloudy, rain.
27	27	32	28	28,797	nw.	Snow, cloudy.
28	26	30	32	29,183	calm.	Clear, cloudy.
29	37	33	30	29,245	do. west.	Cloudy.
30	32	40	34	29,098	do. east.	Cloudy.
31	33	34	34	28,951	do.	Cloudy.
	32°	40°	35°	29.105		

Mean height of Thermometer,	36 14-31°
Greatest height of Thermometer,	67°
Least height of Thermometer,	7°
Mean height of Barometer,	29.105 inches.
Greatest height of Barometer,	29 600 inches.
Least height of Barometer,	28.680 inches.

FOR FEBRUARY.

Date.	Thermometer. Sunrise	3 P. M.	Sunset.	Mean of Barometer.	Prevailing Winds.	General Remarks.
1	34°	40°	39°	28,835	calm.	Clear.
2	27	23	13	29,080	nw.	Cloudy, rain.
3	1	21	21	29,217	nw.	Clear.
4	22	30	32	29,080	west.	Cloudy, Snow.
5	30	39	28	29,160	sw.	Cloudy.
6	25	43	39	29,027	calm.	Clear.
7	38	50	48	28,920	do. sw.	Clear.
8	49	28	22	28,510	sw. & w.	Cloudy, rain, snow
9	12	25	23	29.150	north.	Clear, cloudy.
10	16	30	24	28,995	calm.	Clear.
11	16	23	16	29,053	sw.	Clear.
12	10	20	15	29,215	west.	Cloudy.
13	8	21	14	29,173	west.	Snow, clear.
14	2	21	17	29,243	nw.	Clear.
15	4	26	22	28,995	west.	Clear.
16	23	31	20	28,840	nw. & w.	Clear, cloudy.
17	5	25	22	29,025	nw. & w.	Clear.
18	22	32	29	29,150	sw.	Clear, cloudy.
19	25	40	34	28,862	se. & ne.	Cloudy.
20	2	18	14	29,120	variable.	Cloudy, blustering
21	3	28	24	28,965	sw.	Clear, cloudy.
22	13	32	24	29,180	calm.	Clear.
23	8	32	30	29,300	sw.	Clear, cloudy.
24	32	42	44	28,872	east.	Rain, cloudy.
25	45	57	53	28,245	east.	Rain.
26	33	35	32	28,940	w. & sw.	Cloudy.
27	33	33	24	28,845	ne. & nw.	Snow.
28	19	32	21	29,040	variable.	Cloudy, clear.
	19°	31°	26°	29.001		

Mean height of Thermometer,	25°
Greatest height of Thermometer,	57°
Least height of Thermometer,	1°
Range of Thermometer,	56°
Mean height of Barometer,	29.001 inches.
Greatest height of Barometer,	29.300 inches.
Least height of Barometer,	28.245 inches.
Range of Barometer,	1.055 inches.

FOR MARCH.

Date.	Thermometer. Sunrise	3 P. M.	Sunset.	Mean of Barometer.	Prevailing Winds.	General Remarks.
1	12°	32°	30°	29,080	nw.	Cloudy.
2	22	38	34	29,125	west.	Cloudy, clear.
3	38	51	41	29,145	sw.	Cloudy, rain.
4	41	50	48	28,970	sw.	Cloudy, rain.
5	42	40	36	29,017	ne.	Rain, cloudy.
6	18	40	32	29,325	calm.	Clear.
7	24	44	40	28,935	calm.	Clear.
8	32	48	42	28,975	calm.	Clear.
9	30	52	50	28,940	calm.	Clear.
10	42	56	54	28,890	sw.	Clear.
11	46	56	52	28,900	ne.	Clear.
12	46	44	27	28,925	nw.	Snow.
13	15	36	32	28,990	nw.	Snow.
14	30	42	36	29,100	nw.	Clear.
15	30	40	32	29,120	sw.	Rain.
16	28	36	30	29,080	sw.	Cloudy.
17	28	32	25	29,255	sw.	Cloudy.
18	27	38	31	28,955	sw.	Clear, cloudy.
19	21	37	32	28,870	sw.	Snow, clear.
20	18	35	32	29,140	calm.	Snow, cloudy.
21	26	40	38	29,100	calm.	Cloudy.
22	28	40	38	29,080	calm.	Cloudy.
23	30	44	40	29,055	calm.	Clear.
24	23	36	32	29,125	sw.	Clear.
25	32	48	26	29,100	sw.	Clear.
26	30	50	30	29,070	west.	Cloudy.
27	23	46	42	29,080	nw.	Cloudy.
28	29	42	36	29,110	east.	Cloudy.
29	24	40	35	29,115	ne.	Clear.
30	32	50	44	29,000	south.	Clear.
31	43	54	50	28,980	south.	Cloudy.
	29°	43°	37°	29,050		

Mean height of Thermometer, - - -	36½°
Greatest height of Thermometer, - - -	56°
Least height of Thermometer, - - -	12°
Range of Thermometer, - - - -	44°
Mean height of Barometer, - -	29,050 inches.
Greatest height of Barometer, -	29,325 inches.
Least height of Barometer, - - -	28,870 inches.
Range of Barometer, - - -	.455 inches.

NOTICES &c.

We have received the following publications, viz.

The New York Medical and Physical Journal, Nos. I. II. and III. new series. (In exchange.)

The American Journal of the Medical Sciences, Nos. III. IV. V. and VI. (In exchange.)

The North American Medical and Surgical Journal, No. XIII. January, 1829. (In exchange.)

The Western Journal of the Medical and Physical Sciences. Nos. I. to XI, inclusive. (In exchange.)

Annual address, delivered before the Medical Society of the State of New York, February 4. 1829. By T. Romeyn Beck M. D. President of the Society. (From the author.)

A Discourse on Intemperance; delivered at Cincinnati, March 1st 1828, before the Agricultural Society of Hamilton County, and subsequently pronounced by request, to a popular audience. By Daniel Drake, M. D. (From the author.)

A defence of the experiments to determine the comparative value of the principal varieties of fuel used in the United States, and sold in Europe. Containing a correspondence with a committee of the American Academy of Arts and Sciences; their report, and remarks thereon; and animadversions on the manner in which the trust confided to the Academy by Count Rumford has been managed. By Marcus Bull, member of the American Philosophical Society. (From John Vaughan Esqr.)

A short reply to a Pamphlet published at Philadelphia, entitled, "A defence, &c." as above. By one of the committee of the American Academy. (From the Committee.)

An answer to "A short reply to 'a defence of the experiments to determine the comparative value of the principal varieties of fuel &c.' By one of the committee of the American Academy," (published in Boston.) By Marcus Bull, member of the Philosophical Society &c. (From John Vaughan Esqr.)

Some notices of Kentucky, particularly of its chief town Lexington. By Hamilton. (From the author.)

An Oration delivered before the central Medical society of Georgia, at its annual meeting in Milledgeville, on the 2d December, 1828. By Alexander Jones, M. D. (From the author.)

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THE TRANSYLVANIA JOURNAL

OF

MEDICINE

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VOL. 2. AUGUST, 1829. NO. 3.

ORIGINAL COMMUNICATIONS.

ART. I.—*Thoughts on the Structure and Dependencies of the Science of Medicine.* By CHARLES CALDWELL M. D. Professor of the Institutes of Medicine, and Clinical Practice, in Transylvania University.

BY structure and dependencies, on the present occasion, is to be understood the same that those terms import, when used with a reference to any other subject. They embrace, in their meaning, the materials or elements of which the fabric or composition is formed, their arrangement and connexion, the appearance they present, and the aid and support which they give to each other. Under this view of it, I trust it may be permitted me to believe, that my theme, if fairly and skilfully presented, and treated with judgment, may be made to exhibit an aspect of interest. As it is meant to be somewhat of a comment on the elements of medical science, it will contain, of course, an exposition of the components of a medical education.

The general branches that enter into the composition of medical science, although comprehensive, in their scope, and composed themselves of several subordinate ones, are not nu-

merous. They are, as usually enumerated, Anatomy, Physiology, Pathology, Therapeutics, Materia Medica, and the Practice of physic, including Surgery, both scientific and operative. Such, I say, is the customary division of medical science, into its subordinate branches, although I do not consider it the most natural and philosophical one. But being sufficiently accurate for my present purpose, I adopt it, in preference to proposing any innovation. Under the division of Surgery fall all the branches of Obstetrics. To Physiology and Pathology belong Medical Jurisprudence and Hygieine generally, embracing the preservation of the health of communities, as well as that of individuals. Chemistry forms no part of medical science, except in its connexion with Materia Medica, by means of Pharmacy. All attempts to incorporate it with Physiology, Pathology, and Therapeutics, and make it a part of them, are founded in error, and must be ultimately abandoned. Botany is immediately subservient to Physiology and Therapeutics.

Such are the grand, and some of the subordinate components, of the science and profession of medicine. I have here added the term "profession" meaning to include, under it, the practical departments, which constitute immediately the healing *art*, as distinguished from the *principles*, by which the art is regulated. But a further exposition of these components, as well in themselves, as in their relations, must be attempted.

Anatomy constitutes the foundation of the science. Without it, it could have no existence. Hence, previously to the acquaintance of physicians with it, medicine was purely empirical and tentative. Medical *science* was not yet in being.

Anatomy has been variously divided, by its professors and teachers. It consists essentially of two branches; that which exhibits the structure of the body, as composed of organs; and that which gives the structure of the organs themselves. Here, chemistry again comes in, to play its part; and by an entire resolution of organization, affects to communicate to us some knowledge of the elements of which our tissues themselves are composed. But that knowledge, whether true or false, has given us as yet, no facilities or advantages, either in the science

of health, or in the philosophy or treatment of diseases. On the contrary it has often given currency, for a time, to folly and mischief. Of this were it requisite, many deplorable instances might be mentioned. That branch of anatomy which unfolds the composition of the body, as formed out of organs, is called demonstrative or descriptive. That which unravels completely the structure of the organs, is still further divided into two minor branches. Of these, one exhibits the tissues of which the organs are composed, and is termed *Analytical* Anatomy; the other, the structure of the tissues themselves, and is denominated *minute*. With anatomy, thus analyzed and expounded, it is necessary that every physician be acquainted. Although it relates to the animal body, only when dead, it is essential to a knowledge of its living functions; and also, in a particular manner, to operative surgery.

The branch, which, in a healthy condition of the system, this knowledge of functions constitutes, is termed Physiology. Considered in itself, and its numerous relations, it is extensive and complicated, interesting and important, in a very eminent degree. Embracing an acquaintance with all the functions, it is as really the life and soul of medicine, as it is closely connected with the life of the body. Without it, all would be dead, uninviting, and fruitless. With it, the reverse of this is true.

This is not to be regarded in the light of a mere panegyric, pronounced unreflectingly, from an impulse of feeling, on a favourite branch of science. It is a truth which the internal evidence of the subject sustains, and which both enlightened physicians and public institutions are beginning to recognise. It is more than twenty years, since the writer of this article asserted openly, and resolutely contended, that the school of Philadelphia would be compelled to introduce into its organization, a distinct chair of the Institutes of Medicine which at that time it had not. What was treated as conjecture or unwarranted assertion then, is history now.

But Physiology does not consist alone in a knowledge of the healthy functions of the body, considered in an individual and

insulated capacity. It regards and embraces them, in an associate capacity; in their connexion with, and their influence over each other, and also in their influence on the system at large. It considers the functions and their organs, in a healthy condition, as contributing to the well-being of each other, through the medium of sympathy, and as constituting, in the aggregate, the health of the system.

Physiology goes still further. It considers the sound functions of the body, in their relation to the causes that render them so; to wholesome food and drink, salubrious air and healthful situations, the suitable time, kind and amount of exercise, the proper time and duration of sleeping and waking, the salutary state of the passions and emotions, the temperature and general character of the weather, and to every thing that can contribute to the preservation of health.

Nor does Physiology stop here. It contains many beautiful and instructive analogies between the functions of man and those of the lower orders of animals, and even of vegetables. This is comparative physiology, without which the general science is not merely incomplete, but exceedingly defective. By such analogies it develops much additional knowledge. That physiologist, who confines his attention and researches to the narrow sphere of the functions of man, will never be worthy of the title he bears, or the interesting science he so unskilfully cultivates. Much light is thrown on all things, by contrast and comparison. Nor is this truth more peculiarly applicable to any branch of science, than to that of Physiology. Let him who is cultivating it, study well the functions of *all* kinds of living matter, if he wishes to be completely versed in the functions of *any*.

The more thoroughly and minutely to understand his science, the physiologist, if he be already enlightened, and liberal in his views, will inquire strictly into the influence of temperaments, habits, modes of life, and even of the climates and regions where individuals are born, or in which they may reside. Such influence is well known to modify materially the functions of health, no less than the nature and character of

disease. Indeed it is chiefly through the former, that the latter can be affected.

It is scarcely necessary to repeat, what has been already suggested, that without a knowledge of anatomy, true physiology could have no existence. Hence the connexion between them, and the essential dependance of the latter on the former.

Nor is pathology less closely connected with physiology, or less positively dependent on it, than the latter is on anatomy. A fair interpretation of the term will verify this assertion; and show, that, in the nature of things, it must be correct.

Pathology consists in a knowledge of the same functions and structures with physiology; but they are now in a deranged condition; they are excessive, defective, or wrong in action. No new function or structure is created. But every existing function may become deranged. Carry healthy action to excess, it is no longer healthy; reduce it below the standard of health, it is, in like manner, morbid; change its nature, and disease is also induced. Health and disease are the only genuine interpreters of each other. It is too obvious, then, to be questioned, that without an accurate knowledge of the functions in health, no one can tell, by the phenomena they exhibit, whether they are in a diseased condition or not. Such information can be attained only by comparison; and the phenomena presented by the healthy functions constitute the standard with which alone the comparison can be rightfully made.

For the sake of illustration, let us suppose a practitioner of medicine ignorant of the evidences of a sound condition of the liver. From what source will he derive his knowledge of the morbid condition of that viscus? To him, no such source of information can exist. In his view, the liver, in health and disease, will be the same. He knows neither the healthy colour, quantity, nor consistence of the bile. Whence, then, can he pronounce, that that fluid is vitiated? Nor does he know even the feelings of a sound, contrasted with those of a diseased liver. To all evidences, then, of hepatic derangement, he is a stranger.

Is he ignorant of the manifestations of a healthful condition of

the stomach, the lungs, the kidneys, or the brain? Equally ignorant must he be of the manifestations, which, in those organs, indicate disease. To the skin, the muscles, the intestines, the eyes, the bones, and every other organ of the body, similar remarks are perfectly applicable. Their healthy condition and functions must be known, as an indispensable preliminary, to a knowledge of their diseased condition. Universally, then, physiology is the only foundation of pathology; and is as essential to a knowledge of it, as is an acquaintance with the alphabet, to an ability to read. I repeat, that physiology and pathology alone interpret each other.

To render this matter still more free from misapprehension or doubt, let us recur to the liver. Without changing it, in any other way, render the secretory action of that organ excessive, and it is morbid, on account of its excess; render it defective, that defect is disease; alter it, in its nature, and it is equally morbid.

Thus, at sea, a tempest and a calm are atmospherical phenomena, and are yet deviations alike from a favourable breeze; and a tornado is but a perverted movement of the air.

Pathology, is nothing but physiology deranged. Nor is the derangement confined merely to the morbid action of the part primarily affected. It extends also to the sympathies of parts. It has been stated, that the healthy and natural action of one organ contributes, by sympathy, to the natural and healthful action of others, and thus indirectly to that of the whole system. Derange the action of the same organ, and the scene is reversed. Instead of benefitting other organs, by sympathy, it now injures them. It produces in them a condition no less foreign from that of health, than its own. Nor does it injure merely the organ it had previously benefitted. It sometimes throws its morbific influence *immediately* on parts, which it had seemed before to reach more *circuitously*. Ultimately the whole system suffers, by morbid sympathy.

Physiology and pathology constitute the only foundation of therapeutics. Without a competent acquaintance with these,

the practice of physic is wanton quackery. A brief exposition will verify this assertion.

Therapeutics constitute the science of indication and prescription. Its object is to unfold the principles, and point out the mode, of converting pathological into physiological phenomena; the diseased into the healthful condition of an organ. But it would be superfluous to add that without an accurate knowledge of both the diseased and the healthy condition, this object can never be attained. Where is the deranged organ situated? Does it act excessively, defectively, or pervertedly? Whether do its vessels contain an excessive or a defective quantity of blood? If the quantity be excessive, whether is it arterial or venous blood? Has the organ sustained, in its solid portion, any marked and deleterious change? If so, of what character, and to what extent? All these points must be thoroughly understood, before the process can be known, by which morbid can be converted into healthy action.

Nor is this all the information, which the occasion requires. The organ deranged is not always *directly* accessible to the remedy necessary to the production of a cure. It must be reached circuitously, through the channels of sympathy. The application, therefore, must be made to a remote organ. And that it may be made successfully, there must exist between the organ to be cured, and that to which the remedy is applied, a very marked and lively sympathy. But the knowledge of sympathies constitutes a part of the science of physiology. So does a knowledge of the peculiar and relative susceptibilities of organs, a circumstance of no small importance in medicine. In settling, moreover, the indication of cure, the physician derives essential aid from an acquaintance with temperament. But this, as already mentioned, belongs also to physiology.

Further; the practitioner must have a knowledge of the particular kind of curative action, by which the morbid condition of the organ is to be changed. Is the condition dropsical? It must be removed by absorption or tapping. Is it congestion alone, or congestion united to a thickening and induration of the solids of the part? Its removal is to be effected

by secretion and quickened circulation, or by those two processes and absorption conjoined. But a knowledge of these points belongs to physiology. So does a knowledge of the agents and impressions, by which circulation and absorption are to be augmented. Nor is it unimportant to know, by what kind of secretion the congestion is most likely to be safely removed. Is it by the secretion of pus? One mode of treatment is called for. Is it by the secretion of bile?—another mode is indicated. By a serous secretion?—another. By perspiration, expectoration, urine, or saliva?—another, and another; the mode of treatment differing according to the secretion to be produced. The doctrines of metastasis and revulsion belong to physiological pathology. But they are also essential in therapeutics. So are sympathy or consent of parts, and the vis medicatrix or recuperative power of living matter. Without a knowledge of these, the profession of medicine would be worse than useless. It would be an evil. But they pertain to the science of physiology. To the same science belong the facts, that irritation produces necessarily a state of congestion in the part irritated, and that these existing in a single organ give rise to general disease. But to sound therapeutics, a knowledge of these facts is indispensable.

To every enlightened physiologist and pathologist it is perfectly understood, that there is a certain point of excitement and action, in the human system, at which alone secretion of a healthy character, and salutary in its effects, can ever be produced. If the excitement and action be either above or below this, which is properly denominated the secreting point, there is either no secretion, or one vitiated in character, and useless, at least, if not injurious, in its operation and issue. In physiological pathology, I say, these facts are universally recognised. And I need scarcely add, that, without a knowledge of them, therapeutics would be lamentably defective.

Most of what has been said in relation to therapeutics, is equally applicable to the Practice of physic. For the practice is nothing more, than therapeutics carried into action. Hence it has been correctly enough denominated, practical thera-

peutics. But to the practitioner of medicine, an accurate acquaintance with another branch is equally essential. I mean materia medica.

Thus, well trained in anatomy, physiology, pathology and therapeutics, and furnished with weapons from the store of materia medica, the physician is prepared to enter the lists in combat with disease. But if not thus instructed and provided, he is as unfit for conflict, as the soldier without his discipline or his sword.

A competent preparation, then, for the practice of physic is the result of a correct and thorough knowledge of anatomy, the institutes, and materia medica. To this add an acquaintance with pharmacy, and the practical tact, which nothing but observation and experience can give, and the practitioner is as able and accomplished, as his endowments will admit.

It need scarcely be added, that the principles on which surgery and obstetrics are conducted, are the same that govern the practice of physic. They are those which constitute the general philosophy of the healing art; and in which every practitioner of either surgery or medicine should be thoroughly versed.

If the preceding observations generally are true, and there are but few of them, perhaps, that will be denied or controverted, they are susceptible of an application to intellectual derangement, which ought, I think, to attract the attention of physicians.

So defective has been, heretofore, the knowledge of the diseases of the intellect, and so unsuccessful the treatment of them, that they have been often denominated, not, perhaps, unjustly, an *opprobrium medicinæ.* A knowledge of the causes of this, is of unspeakable moment to us. If those causes be insuperable and irremovable, the fact should be known, that we may submit to them, in a spirit of calm resignation. Nor is it less important to be known, if the case be otherwise, that our inquiries on the subject, may be encouraged by the cheering influence of hope.

I am perfectly aware, that it is not only much easier, but

much more common, to find fault and even to point out error, than it is to discover truth, and recommend snbstantial and salutary amendments. This may, perhaps, be as true, in relation to intellectual derangement, as to other subjects. Still I do trust, that causes, not altogether visionary, may be rendered, why that malady has been so little understood, and so unsuccessfully treated; and that ways may be suggested for the attainment of a more correct knowledge of it, and measures for treating it under better auspices. I do hope that a morning of promise has not only dawned on it but is fast brightening, which will lead to a day of triumphant success; and that intellectual derangements will yet cease to be among the *opprobria medicinæ*, and will be found as tractable as other diseases; particularly as other hereditary diseases. For *madness is* usually hereditary; and, other things being alike, it is well known that affections of that description, are much more obstinate, than those which arise incidentally, or from general influences, and are not attributable to a constitutional taint.

There exist, if I mistake not, several reasons, why intellectual diseases have been but little understood. They have been considered, for the most part, of so abstract, subtle, and mysterious a character, as to discourage, in timid and indolent inquirers, all attempts to acquire a knowledge of them. This was more especially the case, in times of antiquity, during the dark ages, and in other periods and places of superstition, when they were regarded as the result of demoniacal influence. In more explicit terms, when those who were subject to them, were believed to be literally possessed of devils.

But even those physicians, who, gifted with more active and resolute intellects, indulging in the exercise of a more liberal curiosity, and intent on higher and more benevolent purposes, did institute inquiries respecting these diseases, directed their attention to a wrong source, and pushed their researches only into a visionary region. They embraced shadows for substances; and occupied themselves in discussing empty notions, instead of inquiring into important realities. They believed mental alienation, in every form, to be of a spiritual character, hav-

ing no connexion with man's material portion. In other words, they believed it to be a *malady of the mind.* I need scarcely remark, that such a research could not fail to prove fruitless. It was a pursuit after transcendentalism, which, from the laws of nature, as respects man, and, as the very term itself indicates, can never terminate otherwise than in disappointment. Between pure spiritualism and medicine there is no affinity. With spirit, in the abstract, the enlightened practitioner has no concern, but leaves it entirely to another profession. He knows nothing of its anatomy, physiology, pathology, or therapeutics. Nor does he believe such attributes predicable of it. To speak of its diseases, therefore, and attempt to treat them, is rank quackery, and must always disgrace the individual who engages in it.

A palpable ignorance, among physicians, of the faculties and healthy functions of the intellect, is another reason of their very limited knowledge of mental derangement. These faculties and functions constitute the physiology of the intellect. Without an acquaintance with them, then, its pathology can never be understood. This is as true, as it is that the diseases of the system generally can be understood only, through the medium of physiology. But more on this topic presently.

Several causes of unsuccessful practice, in mental diseases, may be easily adduced. As already stated, the complaints have not been understood. The treatment of them, then, could not fail to be ineffectual. Correct knowledge is essential to successful practice.

Madness is usually hereditary; a circumstance which has been already noticed, as increasing greatly the intractability of disease. A hereditary predisposition to complaint is as difficult to eradicate entirely, as a hereditary complexion. When it is excited into actual disease, the case is exceedingly obstinate and unpromising. The supposed intractability, moreover, of madness, has had an unfortunate tendency to suppress all vigorous and persevering efforts to remove it. Like other men, physicians do not like to labour in a hopeless cause. When they anticipate neither benefit to their patients, nor honour to

themselves, they are too apt to be wanting in exertion. And, as an excuse for their failure, and to protect their reputation, they comfortably allege, that they are, in no degree, inferior to others, who are known to have been equally unsuccessful with themselves.

But the most fruitful cause of failure, in the treatment of madness, is, that the complaint is generally permitted to become chronic, before any serious attempts are made for its removal. Be the cause of this tardiness, in the employment of medical aid, what it may, the fact is true. Nor is it less true, that all chronic complaints are difficult to remedy; many of them equally so, with madness itself. There can be adduced nearly as many complete cures in cases of chronic insanity, as in chronic rheumatism, or chronic ophthalmia. To render this comparison fair, each of the complaints must be hereditary, or neither of them so. In recent madness, where the treatment is judicious, cures are frequent; not less so, perhaps, than in other diseases, which are considered tractable. Failure, then, arises much less from the nature of the disease, than from the mismanagement of it.

Reasons, which appear to be worthy of much consideration, can be easily assigned, why madness ought not to be more difficult of cure, than other serious complaints.

All diseases are local, in their origin, have a particular seat, during their whole continuance, and are, for the most part, more or less tractable, in proportion as that seat is more or less accessible to the influence of remedies. The seat of madness, as will appear more fully hereafter, is in the brain. According, then, to the facility, with which that organ is affected, and the degree in which it may be controlled, by medicinal agents, must be the certainty and readiness, with which its morbid condition may be changed, and its diseases cured.

There are two modes in which organs may be subjected to medicinal action. The remediate agent may be applied directly to them; or, being applied to a remote organ, it may throw its influence on them, by sympathy. On the latter ground, the brain may be easily reached and commanded. It

is, in a high degree, under the control of the circulatory apparatus, the stomach and intestines, the liver, and the skin. To affect it indirectly then, both certainly and deeply, it is only requisite to produce the necessary affection in them. Thus, blood-letting exerts a powerful influence on the brain. So do emetics and cathartics, especially when they produce copious secretion from the liver; so do opium and other narcotics; and high cutaneous excitement, accompanied by eruptions, is known to have removed very alarming cerebral affections. The brain, moreover, is deeply controlled by moral and intellectual causes. Music, painting, and mechanical employments exercise it agreeably; and, in their action on it, the passions and emotions are a host in themselves. The external senses generally, as well as some that are partly internal, are also channels, through which it may be acted on, with power and effect. In fine, being the centre of sensation, and connected essentially with every part of the body, there is no organ belonging to the system, more powerfully, extensively, and permanently affected, by means of sympathy, than the brain. And it is well known that many more complaints are cured by sympathetic action, than by the direct application of remedies to the diseased organ. Nor, as will hereafter appear, does it seem to be either visionary or extravagant to allege, that, in time to come, measures will be devised to act on the brain directly by remediate agents. Hence there is reason to believe, that, when judiciously studied, and thoroughly understood, its diseases will be as fully under the control of medicine, as those of any other part of the body. Nor will this be owing to the discovery of new remedies; but to the able and skilful employment of those which we already possess. But to enable physicians thus to employ them, a competent knowledge of the anatomy, physiology, and pathology of the brain is essential. Nor is that knowledge difficult of attainment. It may be derived with facility from the study of Phrenology. For, much as that branch of science has been ignorantly condemned, conceitedly laughed at, and sternly denounced, it is the genuine anatomy and physiology of the brain; an exposition of the true structure and functions

of that organ, and nothing more. Instead of being imaginative and visionary, as compared with metaphysics, they are visionary, as compared with it. It is the substance, they but the shadow. It consists of facts, and is a science; they, of conceits and notions, and are but hypothesis.

Having thus alleged, that Phrenology is calculated to augment our knowledge of the philosophy and treatment of mental derangement, it is requisite that I exhibit the ground on which this allegation rests.

According to that branch of science, cerebral anatomy has been, heretofore entirely misunderstood. The brain is not, as it has been generally considered, a single organ. It is an aggregation, or complicated system of organs, unitized, to a certain extent, in feeling and action, by the influence of sympathy. It is divided into three great compartments, the animal, the intellectual, and the moral, each of which is subdivided into single organs, as follows.

The animal compartment consists of the organs of *Amativeness*, or physical love, *Philoprogenitiveness*, or the love of offspring, *Adhesiveness*, or general physical attachment, *Inhabitiveness*, or attachment to place of residence, *Combativeness*, or a propensity to resentment and battle, *Destructiveness*, or a proneness to cruelty and destroying, *Constructiveness*, or an instinctive disposition to build, fashion, or construct, *Secretiveness*, or a propensity to secrecy and concealment, and *Covetiveness*, or a constitutional desire of possession. The situation, which each of these organs occupies, is known; and to each of them belongs the faculty here attached to it.

The intellectual compartment is made up of the organs of *Individuality*, *Form*, *Size*, *Weight*, *Colour*, *Locality Time*, *Order*, *Tune*, *Number*, *Language*, *Comparison*, *Causality*, *Wit*, and *Imitation*. Of these organs, the names indicate sufficiently the faculties attached to them. Their several situations in the brain are known.

Of the moral compartment the individual organs are *Self-esteem*, *Love of approbation*, *Cautiousness*, *Benevolence*, *Veneration*, *Hope*, *Ideality*, *Wonder*, *Conscientiousness*, and *Firmness*. Of these

the localities are also known, and their faculties are designated by the names bestowed on them.

This brief exposition exhibits the anatomy and physiology of the brain, precisely as I would do the anatomy and physiology of the thorax and abdomen, were I to enumerate the several viscera and organs they contain, and specify the peculiar function of each. And it prepares us as well for understanding the pathology and diseases of the brain, as such an enumeration and specification would prepare us for a knowledge of the pathology and diseases of those two cavities. And a fair and skillful demonstration of the contents of the cranium is equal, in that respect, to a similar demonstration of the contents of the thorax or abdomen. But such demonstration of the brain can be made only by the disciplined phrenologist.

No one will contend, that, without an acquaintance with the several organs and their functions, contained in the thorax and abdomen, a physician could understand the nature or treatment of the diseases of those parts. On the contrary, he could not know even the diseases themselves, to which the parts are subject, much less their character and philosophy. No more can he, without a knowledge of the cerebral organs, their faculties and functions, comprehend the maladies of that portion of the system, and skilfully administer remedies for their removal. Unprovided with such knowledge, he is not prepared even to perceive the existence of intellectual derangement.

In the brain, precisely as in other parts of the system, pathology is founded on physiology; it is a derangement of natural function; it is natural function in excess, deficiency or a perverted condition. But that the excess, deficiency or perversion may be perceived, the existence of the function must be known, and its character understood. But those who have not studied Phrenology are ignorant alike of both the nature and existence of the cerebral functions. Nor do they know any thing even of the organs which perform them. For such individuals then, to understand the *pathology* of the brain is impossible. They are ignorant of its foundation and elements; and must be, therefore, *a fortiori*, ignorant of itself. As well may he, to whom both the

names and functions of the organs contained in the female pelvis are unknown, profess an acquaintance with the diseases of that cavity. Hence the difficulty which physicians always experience in describing mental derangement, and in many cases, in even deciding whether it exist or not. These difficulties and doubts are often sources of great embarrassment and mortification to the faculty, in courts of justice, when they are summoned to testify, in cases of medical jurisprudence.

But to the well disciplined Phrenologist, no such difficulty ever occurs. He is acquainted with the elements of intellectual derangement, and can, therefore, promptly recognize them, when united, and manifested in action. He does not refer insanity to any morbid affection of man's spirit; of his immaterial, indivisible, and immortal part. He knows it is seated in some portion of the brain, and is as certainly, immediately, and necessarily the result of an unsound condition of that organ, as hepatitis is of an unsound condition of the liver, or peripneumony, of the lungs. He considers it not only erroneous, but eminently absurd, to contend for the existence of disease in the spirit, that condition being predicable only of a compound and mutable substance. Being well acquainted, moreover, with all the faculties of the brain, and their modes of healthy manifestation, he is prepared to detect any deviation from those modes. But in such deviation consist the evidences of mental derangement.

Nor does the knowledge of the Phrenologist terminate here. He not only perceives that derangement exists, but he knows what faculty or faculties are affected. He is also acquainted with the local situations of the organs of these faculties. Hence his competency to indicate the immediate seat of the complaint, an advantage which no other physician possesses. A few examples by way of illustration, will render this topic sufficiently clear.

But it is not unimportant previously to observe, that intellectual derangement is always *partial*, as to its effect, and, therefore, *local*, as to its seat in the brain. In no instance, however deplorable, is every faculty of the intellect shattered. In some

of his perceptions, every lunatic is sufficiently correct. Nor is it without interest to remark, that it is the knowing faculties which are usually sane. The individual affected recognizes readily, and with sufficient accuracy, the external qualities of things. He is perceptive of the *form*, *size*, *colour*, and *weight* of objects. His Individuality is, for the most part, sound. He can, therefore, distinguish one object from another. He knows his friends, and enemies, and recognises the different inferior animals. His perception of music is but little impaired, he retains, to a considerable extent, his acquaintance with numbers, and his knowledge of language is rarely much affected. His perception of place is more frequently deranged.

But, by far, the most usual seat of madness is in the organs of animal propensity, moral sentiment, and reflection. The truth of this, a deliberate examination of the insane will confirm. It must not, however, be forgotten, that other organs than those which constitute the immediate seat of the disease are affected by sympathy. For sympathy between the cerebral organs is very powerful. And when the complaint is of long standing, an affection which was originally symptomatic, is rendered idiopathic, through the medium of sympathy. The complaint, therefore, purely local, at first, becomes, by time, not only more confirmed, but more general. Hence, the infinite importance of attacking it early with remediate agents.

The phrenologist, it is repeated, is able, not only to detect the existence of madness, from the deranged manifestations of the faculties, but to indicate its immediate seat in the brain.

Is the disease erotomania, satyriasis or an approach to it, in the male, and nymphomania, in the female? the phrenologist knows that its seat is in the cerebellum. Does it consist in a ferocious propensity, periodical or perpetual, to unprovoked battle, mischief, and murder? The seat must be looked for in the region of the ear, and just behind it. Has it been induced by grief, on account of the loss of children or friends? Its seat is in the back and lower part of the brain, in the organs of Philoprogenitiveness and Adhesiveness. Is insanity manifested by a wanton and ruinous giving away of property? It has probably a

two-fold seat; at least two organs would seem to be affected, Benevolence and Covetiveness; the former being excessive, the latter defective, in excitement and action. Is it evidenced by wild and extravagant emotions of religion? The organ of Veneration is diseased. Is the complaint melancholy madness? Hope and Cautiousness are deranged; as is also some other organ, which determines the peculiar kind of melancholy. That organ the practised phrenologist will easily and promptly discover. Has the affection been brought on by remorse, on account of guilt? Its seat is in conscientiousness. Is it nostalgia? Inhabitiveness is in fault. Is the reasoning power deranged? The malady has its seat in Comparison and Causality. Is the deranged individual possessed of a belief in the presence of supernatural beings? Do malignant ones annoy and terrify him, or do beneficent ones hold with him a friendly intercourse? His organ of wonder is the seat of the complaint. The immediate position of these several organs in the brain, is indicated in phrenology.

It is not unimportant to observe, that the form of the head not unfrequently aids the phrenologist in his inquiry into the immediate seat of madness. Is the complaint erotomania? The organ of Amativeness is unusually developed. Is it ferocious insanity, with a desire to kill? Combativeness and Destructiveness are large. Is it wild religious madness? Veneration is predominant. Is it melancholy? Cautiousness is large and Hope but moderate, perhaps small. Has it been produced by the loss of friends or children? Adhesiveness and Philoprogenitiveness are conspicuous. Other forms of madness are accompanied, in like manner, by corresponding developments. Hence, no one of observation can visit a mad-house, without perceiving that there is something peculiar in the forms of the heads of the insane.

Thus can the phrenologist run over all the modifications of mental derangement, and point out the organs in which each of them is situated. The practical advantages derived from this knowledge of the seats of the complaint, will be mentioned presently. It may be stated, however, now, as a principle in medicine, that where this knowledge is wanting, practice is nothing but blindfold experiment. That physician who attempts the

treatment of a disease, of whose seat he is ignorant, exposes, at once, to the hazard of being lost, his own reputation, and his patient's life. It can scarcely escape observation, that out of this accurate acquaintance with mania, must arise, for the various modifications of it, names much more appropriate, than those which they have heretofore borne. Each name may be rendered explanatory of the modification to which it is given. Thus will nomenclature be made a source of science.

The Phrenologist has yet another means to aid him in attaining a knowledge of the seat of madness. He knows the natural relation which exists between certain causes and particular organs of the brain. He knows, I mean, what organs certain causes are calculated to injure. An acquaintance with the exciting cause of madness, then, which is often attainable, will assist him not a little in discovering the seat. Has it been induced by disappointment in love, friendship, business, or ambition, by the loss of children, or by remorse for crime, the phrenologist is apprized of the organs that suffer. The same is true of madness excited by other causes.

But it is not sufficient that we know merely in what organ insanity is seated. That we may be prepared to treat the complaint with skill and effect, we must have a knowledge also of the peculiar morbid condition of the organ. Without this, we are ignorant of the change required, to convert it into a sound condition.

Nor does the knowledge appear to be difficult of attainment. Taking, for our guide, sound analogy, and *post mortem* examinations of the brains of the insane, we acquire it with ease, and sufficient certainty, to serve as a very reasonable ground of practice.

Do the organs affected act with preternatural vigour, as in erotomania, and ferocious, combative, and destructive madness? They are in a state of arterial congestion, if not of actual inflammation. Are they deficient in action? The congestion is venous, they are compressed by a purulent or a serous secretion, or by some other substance, they are in a state of mere torpor, without congestion, or they are diminished in size, by excessive absorption. The membranes that envelope them are sometimes pre-

ternaturally thick and firm. This condition of the parts is always preceded by arterial congestion. Each of these peculiar conditions of the brain will require a peculiar mode of treatment.

I am aware that we are told by those, who consider madness as a disease *purely mental*, that there occur many cases of it, as dissection proves, in which no cerebral derangement exists. This is a mistake. No such case has ever presented itself. Nor is it in the nature of things that it ever can. As well may we look for a case of peripneumony, without an affection of the lungs, or of hepatitis, with an unaffected liver.

It is possible that in cases of recent madness, this exemption from cerebral derangement may *seem* to dissectors to be true. But it *only seems*, and is not so. Nor is the fallacy difficult to explain. The case of insanity is recent, and, therefore, acute. The vessels of the deranged cerebral organ are surcharged with arterial blood; but, as yet, the *structure* of the part is not altered. After death, the heart having ceased to throw blood into these vessels, they contract, and empty themselves of their contents. Hence, before the dissector has laid it bare, the organ has resumed its natural colour; and, its structure being unchanged, its appearance is healthy. Nor is this all. Many dissectors are so deficient in their knowledge of the natural and healthy aspect of the brain, as to be incompetent reporters on the subject of its condition. Being unacquainted, moreover, with the actual seat of the kind of madness to which the case under dissection belongs, their examination is conducted under great disadvantages. They search, at random, for morbid appearances, and are disappointed; while, a phrenologist, apprized of the spot, to which his attention should be directed, is much more likely to prove successful. Besides, until instructed by the labours of Gall and Spurzheim, anatomists were incompetent to a well managed dissection and examination of the brain. No wonder, then, that their reports have been unsatisfactory.

But in chronic cases of insanity, the brain is always found to be deranged. It is preternaturally injected with blood, its membranes are thickened and condensed in texture, or perhaps ossified, it is itself unnaturally hard or soft, its fibres are palpably

and even greatly increased in thickness, or it presents secretions of a morbid character. Its entire volume is sometimes much diminished, and the cranium equally augmented in thickness. In such cases, the cranium is, at times, spungy or cancellous in texture, and very deeply surcharged with blood. At other times it is nearly as bloodless and solid as ivory. Such are the reports of competent inquirers into the condition of the brain, in chronic madness. Many of them the writer of this article has himself seen verified.

We are told that "nervous irritation" alone, without any perceptible change in the condition of the parts, will produce madness. The view intended to be conveyed by this expression is to me not only unsatisfactory, as a ground of conviction and belief, but unintelligible. It exhibits a supposition, not a fact; something to be proved, not a position already proved. Assert it as a fact; I reply unequivocally I do not believe it.

"*Ubi irritatio, ibi fluxus.*" This is an axiom in physiology. Exceptions to it are very rare; and when they occur, they can be readily explained in perfect consistency with it. Does the irritation amount to an impression or a shock so overwhelming as to destroy life suddenly? In that case, no fluxion to the part, or congestion in it, takes place. Vital action, the cause of the fluxion and congestion, is too soon extinguished. But let the irritation continue a sufficient length of time to produce madness, call it "nervous," or give it any other name, it will be marked by congestion. As far as research has shed light on the subject, it authorizes the belief that arterial congestion accompanies every morbidly irritable condition of the brain and nerves—congestion in the nervous or cerebral substance itself, or in the membranes that surround it.

Having informed himself of the immediate seat of madness, and of the morbid condition of the cerebral organ which constitutes it, the physician is as competent to the rational treatment of it, as he is to that of any other complaint, to which the system is liable. He is prepared to use remedies, both general and local, according to the best established principles of therapeutics. But, without such information, all practice, in intel-

lectual derangement, is palpably empirical. In any other disease, the entire medical world so proclaim it. Let the trial be made, and the issue noted.

A case of peripneumony occurs, and a physician is called to it, who knows neither its seat, nor the condition of the organ which constitutes it. He even imagines the complaint to be seated in some subtle, abstract essence, called *anima medica*, distinct from matter, and cannot be induced to believe, that it has any connexion with the body of his patient. It need scarcely be observed, that every one would instantly pronounce him a quack, unfit for his profession, and unworthy to be trusted. But no less inefficient and unworthy of confidence, in a case of insanity, is that practitioner, who considers the disease as purely mental, and knows nothing of its immediate seat in the brain, or of the condition of the organ which constitutes its seat. Of all the notions in medicine, there is none more silly and unprofitable, than that of the existence of disease in the human spirit. With just as much meaning might we speak of the disease of an angel, or any other celestial being. Such a thought may, perhaps, be tolerated in poetry, or romance; but not in philosophy or common sense.

It has been said that when the immediate seat of madness is known, local remedies may be advantageously used, in the treatment of it, in aid of general ones. An instance, illustrative and confirmatory of this, occurred, a few years ago, in Paris.

A young man was severely attacked by erotomania. For some time, all efforts to remove the complaint were unavailing. Gall was, at length, employed, as consulting physician. Considering the disease as consisting in high excitement and congestion, perhaps, inflammation, of the organ of amativeness, after directing the patient to be copiously bled, from the jugular vein, and as copiously purged by the administration of a powerful cathartic, he had leeches first, and afterwards ice, applied immediately over the cerebellum. The issue was, a speedy and entire extinguishment of the complaint. Is the disease recent, and seated in any other of the cerebral organs? Reduce the general action, by general remedies, such as bleeding, purging, puking,

nauseating medicines, quietude, and a restricted diet, and, over the organ affected, apply, according to indication, leeches, ice, or a blister, or all, perhaps, in succession, and there is ground to hope for a successful result. Copious purging especially when the abdominal secretions are correct, is peculiarly important in mental derangement. A spontaneous diarrhœa always suspends, and has not unfrequently cured the complaint. During an attack of cholera morbus it is also suspended. These are but indications of nature, at once to instruct and admonish the physician. Let him follow the example, and good will be the issue. Let him excite an artificial diarrhœa, with occasional nausea and vomiting if required, and persevere in them a sufficient length of time, and success will often reward his practice.

Nor is it extravagant to allege, that the time is not far distant, when another remedy of a local character, still more powerful than any of those just mentioned, will be successfully applied in the treatment of insanity. I allude to the trephine.

Of the successful application of that instrument, in the treatment of several cases of epilepsy, by Professor Dudley, the public has been already informed, in the Transylvania Journal.

The object of the fortunate operations performed was the removal of the cerebral irritation which produced the disease. And it *was* removed, and the disease along with it.

But cerebral irritation is the cause also of madness. Why, then, not remove it, by the same means. Let those who can answer the question satisfactorily do it. I can only reply to it, that I do not know any solid reason why it may not be thus removed. On the contrary, there appear to be reasons, why it may.

The project is not entirely new. It has been for some time entertained, and often discussed. And the want of a knowledge of the spot on which to apply the trephine, has alone, perhaps, prevented the application. But that want is supplied by phrenology. The point of irritation is indicated by it. The leading objection to the experiment is, therefore, removed.

The operation, being free from hazard, can do no injury. That it will increase either the violence or obstinacy of insanity,

is scarcely possible. On the contrary, on the principle of mere counter-irritation, it promises fairly to mitigate both. Injuries on the head, produced by accident, have relieved madness, if they have not permanently cured it. In some instances they *have* cured it. So have wounds on other parts of the body. Much more likely are they, then, to have the same effect, when skilfully applied as remediate agents.

But the chief advantage to be anticipated from the operation, will arise from the opportunity it will afford, to examine freely the condition of the brain, and employ such means of cure as may be indicated by it.

Is any one, by way of objection, inclined to allege, that the free examination of the brain here suggested, will put in jeopardy the life of the patient? I reply, that the allegation is unfounded. It is now well known that the human brain, especially the upper portions of it, may be much and long exposed, and handled not only freely, but very roughly, without destroying the life of the individual, or even materially injuring his health. Large masses of brain are often *lost*, and life still preserved, and neither health nor strength seriously impaired by the accident. The contents of the cranium may be much more safely treated with freedom and boldness, than those of the thorax or abdomen.

A very large portion of the brain is appropriated not to the purposes of life, but of intellection. And fortunately this is the very part in which madness is seated. If this portion were essential to life, disease in it would kill, as is the case with disease in the heart and lungs. But many madmen live to an advanced age, in the enjoyment of good health. Hence, the part of their brain affected is not *vital*, but only *intellectual*. When deranged, therefore, it may be exposed, examined, and treated as it may require, without any hazard of destroying life. This is fact, not conjecture, and opens to us new views, and brighter prospects, as to the result of our practice in intellectual derangement.

It is now known that pressure, applied with judgment, and skilfully regulated, is the most efficient agent in the removal of

congestion. It is also known that such pressure, gentle in degree, can be applied with perfect safety to the brain.

In madness, from congestion, then, what can be the objection against a free exposure of the congested organ, by means of the trephine, and an attempt to remove the malady by pressure?

Has pressure failed? To the able, bold, and independent practitioner, who thinks for himself, and, untrammelled by authority, endeavours to adapt his means to his purposes, other remediate expedients will occur. Why not apply leeches, fomentations, and other soothing and discussive remedies to an irritated, or an inflamed brain, as well as to an inflamed eye, or any other portion of the body, in a similar condition? If cold water and ice be applied advantageously to the denuded scalp, why not still more advantageously to the denuded brain?

The operation of the trephine having disclosed the nature and character of the complaint, nothing remains but that the physician devise, by his sagacity and skill, the means to remove it.

In what is called the moral treatment of madness, a sovereign expedient is, to withdraw the attention of the patient from the object, respecting which he is deranged, and fix it, as constantly as possible, on some other. Is he deranged on the subject of music? Engage, and interest him in mathematics, experimental philosophy, chemistry, or some mechanical employment. Is he deranged on mathematics or mechanics? Direct his attention by every possible expedient, to music, painting, poetry, or geography? Is his complaint religious madness? Engross his attention in cheerful and rational conversation, on agreeable topics, in the study of anatomy, or in general observations on the works of creation.

Such, I say, is the character of the moral treatment recommended in madness. It must be observed, however, that it is useful, or even admissible, only in cases of moderate disease. If the complaint be intense, perfect quietude and exemption from impression, is as essential, as it is in the most violent inflammatory fever.

It is not a little gratifying to the votaries of Phrenology to observe, that all these prescriptions are in precise correspondence with the principles of that science; that they are such as grow out of it, and such as it alone can explain. Nor is there, in the explanation, the slightest difficulty; nor, in its result, the least obscurity.

The practice is founded on *counter-excitement*, a principle peculiarly operative in the treatment of disease. Is excitement too high in the organ of *number?* Translate it, in part, to that of *music*, *colour*, or *constructiveness*, by exercising the organ to which the translation is intended to be made. Is it inordinate in *veneration?* Transfer it to *causality*, *locality*, or some other of the knowing or reflecting organs. Is it excessive in *combativeness*, or *destructiveness?* Remove it, by revulsion, to *benevolence*, *conscientiousness*, or *cautiousness*. In this practice, the great desideratum is, to ascertain the sound organ which sympathizes most powerfully with the deranged one, and exercises over it the greatest control. To that organ, when known, let the excitement be translated. The discovery of the controling organ must be the result of observation.

It is in this way that one engrossing passion is made to supplant another. In this way a paroxysm of religion, or the excitement of *combativeness*, *destructiveness*, and *love of approbation*, by entering into military life, has removed the pain of disappointed love. And the excitement of *amativeness* has relieved, for a time, if not entirely cured, actual derangement of other organs.

As connected with this subject, it is not unimportant to observe, that the seat of mental derangement is more frequently in the animal, than in either the intellectual or the moral organs. Hence when the disease is treated in the mode here referred to, the counter-irritation is to be made on the moral and reflecting regions of the brain.

The individual, therefore, who is employed to conduct the moral treatment of the insane, ought to have himself a full moral and reflecting development. Unless he has, he possesses neither the intellect, feeling, nor manner requisite to enable him

to perform his duty successfully. He cannot produce on the moral and reflecting organs of the deranged the necessary impression. Such impression can be made only by the proper exercise and application of morality and reflection. He is capable of neither. I take the liberty to repeat, that the success of the moral treatment of insanity depends, in a high degree, on the circumstances under which it is applied.

It is a revulsive process; the transferring of excitement from one part of the brain to another. It must be conducted, therefore, on the well known principles of revulsion. But when the system is under high excitement, or any deep and absorbing impression, that process cannot be effected. Blister in pleurisy, when the fever is intense, and the effect is injurious. To render it salutary the fever must be reduced, and the disease rendered local, or very nearly so.

In like manner, madness must be moderate, else moral treatment, far from being useful, will aggravate the complaint. Before it is attempted, then, let morbid excitement be weakened.

To the intelligent physician, it would be superfluous to remark, that, on the point referred to, the preceding observations tend to show, that there exists between the treatment of madness and that of other diseases, a perfect analogy.

The views contained in this paper have been communicated, by me, for years, to my phrenological classes. If similar ones have been suggested, by any other writer or teacher, the fact is not known to me. That they are supported by strong probability, and may be fairly entertained, I am compelled to believe. Their practical merit must be determined by experience. To that, therefore, which is but the judgment of time, they are freely submitted.

ART. II.—*On the Utility of Blood-letting in the advanced stages of Fever.* *By* GEORGE W. CAMPBELL, M. D. of Columbia, Tennessee.

THE science or system of Medicine may be compared to the great business of manufacturing, the influence of which is felt throughout the civilized world. It receives support from the humble tiller of the ground, as well as from those, who have the more immediate control of the commercial world. Materials sent from all quarters of an extensive scope of country to a manufacturing point, are converted by the workman's hand into various fabrics, and sent back in a condition suited to the various wants and necessities of the inhabitants of the different sections of country, from which the raw materials were collected.

The science of medicine has to be supported somewhat in the same way. Let the principles, at the fountain head, be as correct as they may; let there be talents and virtue sufficient in the different universities, yet except they be supplied with the materials from the different sections of country, they cannot construct theories, or chalk out a course of practice, to suit the inhabitants thereof. It is then the duty of every member of the profession, who feels an interest in the advancement of the science of medicine, and the alleviation of human sufferings, to be attentive to the collection of such materials, from the sick chamber, and from the bosom of surrounding Nature, as are adapted to such purposes. But those of us, whose lots have been cast in the interior of the country, remote from the fountain heads of professional knowledge; who, from necessity, have to devote our time and attention to the laborious duties of our profession, cannot expect to be of much closet advantage to that profession. Little else then can be expected of us, than a correct detail of such facts and principles, that occur to us in practice, as may be new and interesting to the faculty, and of benefit to the community in general.

It is my design in this paper, to show the necessity and utility of blood-letting in the latter stages of fevers, particularly typhus fever, which I had intended doing by simply relating a few cases, where it was used with success when all other remedies had failed. But before I proceed, I shall stop to make a few brief remarks on fever, so as to bring into view the principles on which such a practice would rest.

As to the doctrine of fever being nothing more than an effort of nature to remove some aggravating cause, I will suspend an opinion, and leave it for those to believe, who are less sceptical on the subject.

When we find the general system involved in febrile action, we are presented with symptoms of broken excitement, loss of balance between the secerning and excerning systems or functions, with derangement of the secretions, &c. When the system has been long under the influence of fever, there is generally decay, or shrinking of the solids of the body, owing to the increased activity of the absorbents, while the secretories are inactive; though to this general rule, I will notice an exception hereafter.

Whether fever be idiopathic, or symptomatic at the commencement, the whole system, sooner or later, becomes involved. Digestion, in common with the rest of the secerning functions, is generally disordered, and frequently entirely suspended. In those protracted fevers, patients die very much emaciated, or convalesce and look like walking skeletons. What is the cause of this rapid melting away of the solids? The absorbents are actively employed in tearing down the structure of the system, while the sources of renovation are cut off.

A question now naturally arises touching the pathological condition of the system, or parts of it, in such cases. What disposal is made of the matter which the absorbents take from the solids of the body, when, during the whole progress of the disease, the emunctories of the body have been locked up? Do the absorbents not throw this matter into the general circulation? They certainly do. Then just in proportion to the decay and debility of the solids, provided the emunctories are not kept

open, will be the increase in the quantum and weight and deterioration of the fluids, which those solids have to keep in motion.

This I conceive to be the state of affairs in those fatal congestions, that so often take place in typhus fever. If we found a patient very much debilitated and reduced, and were convinced that the blood-vessels contained their ordinary quantum of blood, would we not be apprehensive of mischief, and immediately set about diminishing the amount of fluids, so as to prevent congestion and its consequences, rather than attempt to force the solids, already worn down with disease, to keep in motion and bear up under the burthen? If our carriage gets into the mire, and some important part of it is about to give way, would it not be better to take off some of the load, and apply the ordinary, or even less force, than to increase that force with a view of dragging it along? When there exists disease of the system, and the moving powers thereof are crippled, together with an increase of the burthen to be removed, would it not be the better plan to remove a portion of the weight and give the system an opportunity to rally its forces, than make an attempt to repair the injury, while the burthen remains, and is permitted to accumulate?

In the commencement of fevers, we very often have inflammation and congestion to encounter; but they are produced from different causes, and require a different treatment to remove them from that which is found to be the case, in the latter stages of the same diseases.

Fever makes its attack upon a system perfectly healthy, in every respect. The solids become very much excited, and act with great violence upon the fluids they contain, propelling them with increased force and rapidity throughout all parts of the system, until some delicate structure, such as the brain, spinal marrow, liver, stomach &c. gives way to congestion and inflammation. In this stage the action of the solids is too great for the weight they have to move, and the sphere it is to be moved in. And what would be the consequence, if things were thus permitted to go on uninterruptedly? It would be like a vesssel

driven ashore by a gale of wind, with hoisted sails, running the risk of breaking every thing to pieces. But our good old mother, the *vis medicatrix naturæ*, who is always ready and willing to aid and assist when required, immediately commences her operations to curb the enraged solids; this she does by arresting digestion and sanguification, and setting to work the absorbents, with a view to lessen their power, and, at the same time increase the necessity for the exhibition of more power, by adding to the weight in a reverse ratio to the power by which it is to be sustained. To assist nature in this stage of the disease, we bleed, puke, purge, &c. all with a view to reduce the action of the solids, or arterial action, as it is commonly called, and not to lessen the weight or quantum of matter, which has to be kept in circulation, as is the case in the more advanced stages.

In the early period of fevers, we frequently lessen arterial excitement without the benefit of the drain, that is produced and kept up by purging, sweating, &c. We do it by the use of nauseating medicines, cold drinks, cold external applications, bleeding ad deliquium animi, &c. But when the disease becomes protracted, the case is entirely different. Blood, or rather the quantum of fluid in the blood-vessels (for sanguification is arrested,) has been accumulating; the stomach, the bowels, the liver, the brain, spinal marrow &c. are occasionally almost destroyed in a physiological point of view.

When called to the bedside of a patient, who has been laboring under fever for several weeks, extremely emaciated, with a confused wiry pulse, parched skin, low muttering delirium, and other symptoms, which go to convince us, that there is great derangement of the nervous system, debility and suppression of the solids, and an increased accumulation of fluids in the blood-vessels, which, if permitted to remain, will soon extinguish the glimmering remains of vitality, what are we to do? Debility, and stimulants as its remedy, are generally associated; and stimulants are generally resorted to in those cases of which I am now speaking; but, if we depend upon them, we will, but too often, be disappointed. Stimulants, when used at the right time, and in the proper manner, are, no doubt, of great benefit,

and probably save many a patient, that would die without them; but to use them, because there is, or appears to be debility, which is produced and kept up by causes that they cannot remove, is to use them without any benefit, except indeed, the use of them be continued until, and after the disease is removed by other means, or escapes of itself, and leaves the system prostrate, unstimulated by disease, and with too little healthy action, or strength to live without stimulus of some kind—now, and not till now, in my opinion, will stimulants be of any real benefit.

I can conceive of the possibility, and even probability, of patients dying, after they have gotten well, or out of danger, so far as regards the immediate presence or operation of the disease, that was the remote cause of death. When the warfare has been long kept up between life and death, or health and disease, the result is very uncertain. When the system is very much reduced by disease, and the disease leaves it abruptly, it may recover, and it may not. The system may be so low, that the sudden removal of the stimulus of disease, when there is no healthy stimulus supplied, might prove fatal. About this period of fever, would be the proper time for our stimulants to come in, just when convalescence, or death is to take place.

The general effect of stimulants, when exhibited internally, is to increase the kind of action, that exists at the time of their exhibition, whether it be morbid or healthy. Then if we use stimulants at all in those cases, let us exhibit them, when we have brought our patient to the point of convalescence by bleeding, purging, &c.

I have known quarts of strong alcoholic drinks exhibited, in the course of a few hours, to keep the patient from dying, after the crisis alluded to had taken place. I have also seen almost incredible quantities of stimulants given, while disease appeared to control the operations of the system, without the least benefit resulting.

Shall we, in these protracted cases, resort to the lancet with the same object in view, that we would have in removing the same cause, in the early stage of fever? If we did, and it had the intended effect, we would kill our patient. But if we

bleed at this critical moment, with a view merely to lessen the quantum of blood, and be so lucky as to extract large quantities of that fluid, without producing syncope, the result would be glorious. When purgatives, diaphoretics, and the various drugs of the shops have failed to drain the system or bloodvessels of the surplus fluid, I have resorted to the lancet with good effect, when the patient was apparently beyond all hope; when the last struggle appeared to be existing between life and death. This may be considered a hazardous practice, yet I have never regretted bleeding a patient, in this stage of fever. But I have often reflected on myself for permitting the doubts of other physicians, and the prejudices of the friends of the patient to prohibit me from practising it. There is danger in using the lancet indiscriminately, either in the early or latter stages of fevers, more particularly in the latter.

Should we be mistaken, with regard to the true state, or pathological condition of the system, in those protracted cases of fever, and resort to a remedy so decisive in its effects, as blood-letting, the consequences might be serious.

Should we find our patient, and we frequently do, in the latter stages of Typhus fever, nearly exhausted with diarrhœa, bathed in perspiration &c. which ought to convince us, that the bloodvessels have been drained, and that there is a scarcity, rather than a surplus of blood, notwithstanding there may be strong symptoms of congestion, resembling those that exist where there is a surplus of fluids in circulation, and bleed, we should kill our patient.

Symptoms, resembling those of real congestion, may arise from mere irritation; the limits however prescribed to this paper will not permit me to stop here, to notice the subject of irritation: but to keep in view the principles, from which my practical inferences are deduced, I will here state more fully what I have before hinted at, that irritation, and its consequences may be produced, and kept up from too much or too little excitement. When there is great irritability from too much plethora and excitement, depletion would be indicated to prevent, or remove congestion, inflammation, &c. On the contrary,

if there be great irritability &c. produced by too great expenditure, weakness, and a want of blood to afford the necessary excitement to the solids, with an exhibition of the same symptoms that attended the case, where there was too much strength and plethora, depletion would prove injurious, when the proper exhibition of stimulants would calm the agitated frame, and restore the patient to health.

An objection would no doubt be raised to my theory of there being more blood in the veins and arteries, in the latter stages of febrile diseases, than there is at the commencement, or in health, owing to the fact, that in dissecting patients who have died of those protracted fevers, there is generally less blood found in the bloodvessels, than in persons who have died suddenly, when apparently in good health. I have frequently observed this to be the case, and in fact I suspect it is seldom otherwise. But this objection will cease to exist, when we recur to the no less universal fact, that persons die from the immediate effects of some colliquative discharges, as diarrhœa, diaphoresis, &c. Those colliquative discharges may exist, without any external manifestations of them except the sinking of the patient.

The liver, for instance, may open a sluice for the bloodvessels to empty themselves into the bowels, when they might fail to expel the matter thrown into them. A few hours before death, there may be quarts of blood in circulation, the veins and arteries may be full to overflowing, when at, or after death, we might find but gills, and the bloodvessels in a state of collapse.

It is before this colliquative, or melting process is established, that I would use the lancet, to produce, to a moderate and salutary extent, what it produces generally to an immoderate and fatal degree. By the use of the lancet we can extract what quantity of blood we wish, but when those colliquative discharges take place, it is often beyond our power to control them, and the life of the patient is literally carried off through the bowels, kidneys, or the great emunctory of the body, the skin.

Moderate diarrhœa, diaphoresis, &c. whether natural or

produced by medicine, are calculated to produce the same effect we wish to produce by blood-letting. They lessen the quantum of blood and excitement, and the patient will get well, if they do not lessen it too much. When I can succeed in keeping open some of the waste-gates of the system, as the liver, skin &c. by the use of medicines, I prefer it to the lancet. But frequently I find it impossible to do so, and am compelled to resort to the use of the lancet, to get rid of the surplus blood.

What physician is there, who has not witnessed profuse hæmorrhage from the nostrils of patients, who were expected to die almost every hour from those protracted fevers, with immediate benefit, and the commencement of their recovery dated from the moment of the occurrence? Should not those circumstances cause us to pause and enquire into their mysteries, if such they be? Should we be so tenacious of fine spun theories, bound down by the authority and ipse dixit of men, and our own prejudices, as to give a heedless ear to the voice of nature, when she speaks in accents like those?

If an enemy were locked up in a room with us, and the key lost, would it not be better to throw him out at the window, than to risk his remaining with us, under the uncertainty of having the door opened?

Then if an enemy, or disease be locked up in the system, and the key lost, what are we to do? We have made every effort in our power to unlock the liver, the bowels, the skin &c. but have failed, and delay is dangerous, because the disease is preying upon the vitals of the system. When we know where, and what the danger is, common sense will dictate the course best calculated to remove it. If we fail to keep up the right kind of discharges from the liver, (I say the liver because there is generally a determination of blood to that organ, and because it more effectually drains the bloodvessels of their surplus and deteriorated fluids, than any other organ,) and the case becomes protracted, there will be great danger in waiting for nature to perform a cure, by a critical discharge. Then the only hope is in the lancet, by which we can unload the debilitated blood-

vessels, and enable our purgatives, or stimulants, or both as the case may require, to operate kindly and beneficially.

In conclusion then, of my remarks at present, I will only say, that, as a general rule in those continued fevers of from two to six, or more weeks duration, the excretions do not keep pace with the demand for them, and that in a great number of instances, particularly in a country practice, where we can see our patient but seldom, when we depend upon purgatives and other medicines to keep up sufficient discharges, to prevent accumulation of fluids in the circulation, we distress the stomach and bowels, and increase the irritability of the general system.

I will now proceed to detail a few cases, and first I will notice one that I have just treated.

Case. On the 15th of last month, February, I was called to see Capt. S. aged 58, in consultation with the physician who had been in attendance on him for three weeks. I found him very low, he was not able to turn himself in bed or speak above his breath, had a dark dry tongue, parched skin, pulse quick and not very hard.

He had been taking cathartic medicines of a mercurial kind almost every day from the commencement of his sickness, which appeared to have no effect in arresting the fever in its march. I drew nearly a quart of blood from the arm, which, to use his own language, made him feel much lighter, and he shortly dropt to sleep. I then gave a large dose of calomel and aloes, and directed pills of the same to be taken freely every day, so as to keep up consistent discharges from the bowels, and so soon as they failed to purge well, if the fever continued, to draw more blood. As he lived a considerable distance in the country, I did not return to see him. He continued to mend for about six days, when he got worse, and I was again sent for. When I arrived, I found that the attending physician had pursued the course prescribed, with the exception of blood-letting, which he declined doing on account of the debility that appeared to exist.

I found the patient pretty much as when I first visited him. I drew a pint of blood from the arm, and prescribed ten grains

of calomel, four times a day, and oil to be taken occasionally, if the bowels were not well operated upon. I now visited him every two or three days, and continued the same prescription for about a week, when his mouth became a little sore, and his tongue began to lose some of its coat. I curtailed the calomel and gave pills of calomel and aloes, so as to keep up a free discharge from the bowels. There was but very little alteration, apparently for nearly two weeks, when I found his fever give way to the great alarm of his friends, as they thought he would die; then I laid aside calomel, bleeding &c. and he drank pretty freely of wine, and took a dose of salts, every day. He soon got well.

While attending this gentleman, his wife was attacked with the same disease, which he was labouring under, and of which some of his negroes had died a short time before. She took two or three doses of calomel and salts, without doing any good; I gave her an emetic, and directed her to take ten grains of calomel, every two hours, in the day, for two days, which purged her and made her mouth very sore, and cured her without any further ceremony.

Case.—On the 17th September 1825, I was called to see the daughter of Mr. S. aged 13 years. I found her labouring under a fever that was prevailing at the time through the country, and proving fatal in many instances. A remitting, frequently running into what is here called Typhus, fever. She was taken through the common course of puking, purging &c. After the lapse of fifteen days, she was found with the symptoms of one wearing down, in the last stage of Typhus fever. She had a black dry tongue, parched skin, quick wiry pulse, constipated bowels, with low muttering delirium. She was indeed looked upon by parents and friends as past recovery. Purgatives, stimulants &c. had failed to arrest the progress of the fever. I concluded, that it would never do to stand by and see my patient die, while there was yet hope, without making an effort to save her life. This brought me to a serious consideration of her case.

When the hand of disease was first laid upon her, she was a

plump, hearty, rosy looking girl, and now she is the picture of death, reduced almost to a skeleton. I asked myself what had become of all that flesh and blood, she had at the commencement of this fever? Had we drained the system of it by bleeding, purging, sweating &c.? No, she was bled but little, purgation was with difficulty kept up, and taking all the evacuations into consideration, they were not sufficient to have kept pace with the decay of the system. The conclusion then was, that there was too much blood in the veins and arteries for them to manage, in their then debilitated condition.

I told the parents and friends of the patient, that the only hope was in blood-letting, and that possibly, she might die in the operation. I was thus cautious because it was a practice, I had never tried before, and was not so certain what the result of it would be. However they readily consented. I opened a vein in the arm, and drew a pint of dark grumous blood, which did not coagulate. I took the precaution to put my finger upon the orifice occasionally, and stop the flow of blood for a few minutes at a time, to observe the effects the loss of blood might produce upon the pulse. I was probably nearly an hour extracting the pint of blood. The pulse soon became softer and fuller, and it was perceivable to all in the room, that she was benefitted by the loss of blood. The system became more composed. It being now late in the evening, I gave her a scruple dose of calomel, and directed a strong tea of senna and rhubarb root to be used freely, also a little wine or French brandy.

In the morning, I found her much better in every respect: her bowels had been well operated upon. There was still some delirium, and I took about eight ounces of blood, and directed a continuation of the purgative medicine. The last bleeding appeared to afford more immediate relief than the first. I continued the stimulants, with a view to support the system, while undergoing the important change from disease to health, least it might, perchance, fall between the two. She recovered rapidly.

In this case, it is plain to be seen, and the result proves be-

yond a doubt, that there was too much blood in circulation for the bloodvessels to manage, in the situation they were.

The disposable solid parts of the body were rapidly taken up by the absorbents, and thrown into the circulation, when the call upon the vascular system, or the drain kept up from the bloodvessels, was not in a ratio proportionate to the conversion of the solids into fluids. The large emunctories, or wastegates of the system, were locked up, and refused exit to the accumulating fluids.

I could bring forward numerous cases, to prove the efficacy of, and necessity for the use of the lancet, in the latter stages of these protracted fevers; but I will let those already cited, suffice for the present.

Of the exception to the general rule, or law of the economy of the system, observed in disease, which I promised, in the foregoing part of this paper, to notice, I shall say nothing, until I come to notice some of the peculiarities, that characterized the fevers of the last summer and fall, which will be the subject of another paper.

ART. III.—*On the use of the Bandage in the Treatment of Contusions, Lacerations, Effusions &c.* *By* Benjamin W. Dudley, M. D. &c.

CONTUSIONS whether accompanied by a wound of the integuments or otherwise, are characterized by an injury done to the small vessels with an escape of their contents into the common cellular substance, whereby swelling and discoloration of the adjacent soft parts are induced.

Swelling, pain, inflammation, and suppuration constitute the successive morbid states of a part contused, provided the necessary preventive means are not resorted to in due time to interrupt the chain of morbid action. The impression made on the

part where violence is done, is various in degree, notwithstanding the integuments may have escaped injury. The soldier who dies on the field from an injury inflicted by a spent cannon ball, the sailor who is killed by a splinter of his ship striking him on the abdomen or chest, and the husbandman who has an imposthumation consequent to a bruise, present familiar evidences of the truth of this position.

In these as in other instances when the integuments are lacerated, the fate of the patient will depend very much on the force of the morbid impression. When reaction is destroyed, death of the part, if not of the patient, will inevitably ensue; while in other cases as very frequently happens, the principle of reaction, not being properly controlled, transcends the bounds prescribed to living action, whereby mortification supervenes.

It is the province of the practitioner to interpose in these cases and by moderating reaction, to restore the parts to health. Although nothing can be effected for those parts wherein the restorative energies are destroyed by the violence of the impression, yet much of the comfort as well as the safety of the patient can be secured by the skill and assiduous care of the practitioner.

It has frequently happened after violent and extensive contusions, that the patient has been lost by profuse morbid secretions. These are always susceptible of great modification, and in many cases of speedy and effectual correction, by the proper adjustment of pressure. No one who has witnessed the powerful agency of the bandage in surgical diseases, can possibly doubt its controlling influence on suppurating and granulating surfaces. It is a remedy which is destined very speedily to be ranked above all others, where life is in jeopardy by reason of the violence and extent of an injury, the abundance of morbid secretions which are likely to ensue, and the liability of the parts to mortification from excessive action. A contusion so deep in its impression as to destroy all power of reaction is followed by tumefaction and by mortification. By suppurative action and absorption the living are separated from the dead parts; a process too often hazardous and always tedious. When a similar

injury is treated from the first by the bandage, all intumescence is prevented while the first efforts of nature consist in separating, by disjunctive absorption, the living from the dead part, thus curtailing in a most remarkable degree the time occupied in this most important process.

But there is still a higher advantage secured by the application of the bandage in cases of contusion with a loss of the parts from the violence of the injury. Mortification from local violence, although not so alarming in its attendant symptoms as when proceeding from constitutional causes, is notwithstanding much more hazardous to the life of the patient than a similar loss by the amputating knife; and the reason to be adduced is of a palpable kind. The commotion excited preparatory to mortification is often not confined to the mere circumstance of separating from the balance of the system that which is insusceptible of preservation: on the contrary the general morbid excitement induced in many of these cases is so high and so prostrating, that the patient is frequently lost before the process is completed.

A case of injury done the hand, recorded in this Journal vol. 1, page 530, presents in very strong light the efficacy of the bandage. Its tendency to subdue pain and arterial excitement, in those cases wherein sloughing must precede recovery, is clearly ascertained, while its virtues in affording additional security to the patient, and in curtailing the period of confinement, are circumstances of equal commendation.

Under the influence of ordinary treatment we are taught to anticipate danger, as well from tetanus as mortification, in wounds of a lacerated and contused description; while fistulous abscesses are known to harass the patient and embarrass the surgeon who has hitherto possessed no plan for speedily curing these cases. A boy had his thigh dreadfully lacerated and contused in the machinery of a horse-mill, three fourths of the muscles being divided and the bone fractured. The limb was removed, the operation consisting simply in a section of the undivided muscles. Being properly dressed with a bandage, the stump healed throughout by the first intention, the muscles

which had been torn asunder by the wheel presenting not more difficulty in the process of reunion than those which had been divided by the knife.

The controlling influence of the bandage in those cases of contusion in which inflammation has already supervened, is not less decisive, than is tartar emetic in exciting the stomach to expel its contents, when this article is exhibited as an emetic. It is true that the effects of the bandage are less satisfactory in cases of contusion wherein deep seated suppuration has already occurred, and in which the secretion has no exit; as well as in some other cases in which from the irregular conformation of the parts it cannot be made to press equably and effectually upon all points of the surface. In all those however where it can be regularly and equably applied, the aid of no additional remedy is necessary in order to secure the patient against inflammation and all its train of evils.

Its effects on progressive inflammation are instantaneous, while all preternatural enlargement is speedily reduced by the absorbents. Through the agency of the absorbent vessels inflammatory swellings are discussed and the part restored to ordinary magnitude with surprising rapidity under the influence of well adjusted pressure.

By presenting a mechanical obstacle to the influx of blood, the bandage exerts an immediate effect on that sub-system, upon the action of which inflammation depends. Thus exerting an equally decisive influence on the two important systems engaged in all the modifications of health and disease, the bandage claims a character superior to all other remedies whose tendency is directed to the circulatory system. The common consequences of contusions followed by inflammation, the result of neglect or from the use of inappropriate remedies, are, in the bones caries, in the capsules anchylosis, in the cellular substance suppuration, in the muscles unyielding rigidity, in the glands tumefaction, and in the fasciæ painful contraction.

There is scarcely any subject on which practitioners are less divided in their views of practice than in relation to the one under consideration. Poultices, cooling washes, liniments, un-

guents, blisters, with the usual constitutional antiphlogistic means, are invariably relied on as the principal means in most cases of inflammation: not that these are equally valuable as preventives and cures, on the contrary there are among them, remedies of a destructive tendency in particular forms of inflammation. The poultice is a most inappropriate remedy in purulent ophthalmia. An individual of weak and excitable habit, after a severe contusion, is in danger of mortification from great liability to excessive reaction without the power of sustaining action, as another more robust, but rendered weak by loss of blood, or by other evacuations, may be under similar circumstances exposed to mortification because all power of reaction is destroyed. Gun-shot and other contused wounds furnish numerous instances of both, as all practitioners of experience can bear testimony.

In the bandage a preventive to high morbid action in all cases where it can be applied, and a curative remedy of superior efficacy, is presented to the medical community for all those cases of contusion wherein difficulty and danger is encountered from the disposition to excessive reaction: nor is it among other remedies, of less comparative value in those injuries in which all power of reaction is destroyed.

The beneficial influence of the bandage in those cases wherein the principle of reaction is destroyed, (the part being killed) by preventing all tumefaction and inflammation during the process of suppuration and disjunctive absorption, is peculiarly manifest.

What surgeon has not seen patients with sloughing limbs from contused wounds, who were ultimately lost by repeated hæmorrhages? The bandage, by reducing the magnitude of the parts, by curtailing the amount of blood flowing through vessels already passive and inactive, by retaining their divided ends in a quiet state, and in a condition favourable to a permanent reunion, cannot fail to place all such cases in a condition most compatible with ultimate safety. The usual dressings by poultice in cases of the above character, by increasing the

extent of tumefaction, tend to aggravate in no inconsiderable degree the danger of the patient.

The extent and the degree of swelling and inflammation consequent to contusion, must correspond somewhat with the anatomy of the parts and with the extent of injury inflicted. Tendinous expansions and capsules are slow to act, but when inflamed they are seen too often to baffle the skill and energy of the practitioner, and to exhaust the patience of the sick.

Every limb that I have found it necessary to amputate for females in a practice of twenty years, except in one instance, was destroyed by sprains of a slight character, which from want of proper attention progressed insidiously to the final destruction of the joint.

A little girl leaped from a fence and slightly sprained one ankle, which gave so little inconvenience, however, that it was not attended to. For many months she continued to use the limb, complaining at some times of great pain, while at others it seemed to occasion but little inconvenience. Finally inflammation and swelling in the part occasioned so much suffering that she was compelled to use the crutch. Subsequently she became a stranger to quiet and healthy repose, and for the purpose of procuring rest resorted to opium, the use of which was carried to very great excess. Lastly the ankle joint suppurated, and after being confined to bed two years and reduced by hectic fever to a state of extreme weakness, and of great hazard of life under any treatment, it was proposed to amputate the limb. For the first time I was now called to see the patient, and since the removal of the limb presented the only prospect of preserving life, the operation was performed without delay. The limb was taken off a few inches below the knee, and so soon as the patient was dressed and placed in bed, the leg was brought up for examination. All the bones of the Tarsus as well as those of the leg were reduced to the consistency of soft cheese. Even where the leg was amputated, the bones were so much changed by disease as to excite some apprehensions about the cicatrization of the stump. The promise of a successful termination of the case however was soon

made manifest in tranquil sleep, increasing appetite, alleviation of all suffering, and in rapid restoration of strength. The stump cicatrized without difficulty or delay, and in a short time she was enabled to suspend the use of opium, which she had been accustomed to take to the extent of an ounce a week.

A little girl of this town sprained her ankle slightly and continued to walk upon it for several months after, without much inconvenience. Finally she became the subject of a severe and tedious attack of fever, the morbid influence of which was particularly directed upon the injured joint. After the subsidence of the fever, the ankle became unceasingly painful, on account of which various remedies were resorted to in succession during a period of eighteen months. I was then for the first time invited to see the patient and take charge of the case. Upon examination it remained a matter of doubt, whether the deranged state of her general health, or the local affection, called for the most prompt attention. Although suppuration in or about the joint had not supervened, yet its tumid state with the long continued pain in the part, excited great apprehensions in regard to the condition of the bones. With a view to the highest effort at restoring the joint, the part was kept under the influence of well adjusted bandages for six months, while various medicines were prescribed, and great caution in diet enjoined, for the purpose of restoring the general system to health. But notwithstanding all the attention given, it became evident that farther efforts with a view to the preservation of the limb were injudicious, while the constitution of the patient was so much enfeebled as to make it necessary to remove the limb in order to preserve life. Under these circumstances the limb was amputated about three inches below the knee, where the bones of the leg had undergone such material alteration that they might have been divided with a knife. All those of the Tarsus and ankle joint were reduced to the consistence of a pulpy mass, without any appearances of suppuration. With the loss of the limb the sufferings of the patient ceased, while she regained her health and strength in an unusually short period of time.

The two preceding cases exhibit the fatal consequences of imposing too much exercise on slightly sprained joints: in the following, although the joint was destroyed, the limb was preserved.

Mr. N. of the U. S. army sprained his ankle slightly by stepping incautiously from off the side-way in one of the streets of New-Orleans. The injury being looked on as inconsiderable in character, the officer continued in the daily discharge of his duties until from its aggravated state he was incapable of walking upon that leg. Twelve months after receiving the injury the officer reached this place, at which time and for several months previously it gave him much pain.

The whole foot and ankle were swelled, indurated, and very painful; all power of flexion and extension being destroyed. By the assiduous use of the bandage persevered in for several months, the disorganization of the tarsal bones was prevented notwithstanding the foot was deformed and the joint anchylosed.

It is manifest to those conversant with the practice of the profession that the preceding cases are among those not extraordinary in character, while the termination of them all was attributable to the premature use of the joint and to the want of appropriate dressings. The common roller well applied, by curtailing the movements of the joint, and especially all irregular movements, and by giving artificial support to the wounded capsule, will in very many instances secure the part against disease, under the influence of exercise, that would without the protection of the bandage most certainly be followed by destructive consequences.

These cases contrasted with others in which the bandage was applied immediately after the reception of the injury, will offer suggestions in regard to the healthy influence of the bandage in recent cases of effusion and of contused and lacerated wounds.

Miss L. attempted to save herself by leaping from the saddle when the horse was in full speed. Falling on one foot, the bones of the leg ruptured the anterior portion of the capsular ligament and rested low down on the instep. On seeing the

patient half an hour after, I found her pallid, agitated, and in great pain. The foot and ankle were discolored and somewhat swollen, while almost all the capsular ligament appeared to be ruptured. Partial and temporary dressings were immediately put on to prevent swelling and to sustain the parts while returning to her apartments. After reaching home a bandage of proper dimensions was applied from the toes to the knee. Until all swelling was discussed the bandage was renewed daily. A removal of the dressings once in four or five days answered the purpose after all swelling subsided. On the expiration of the third week the joint was restored to health.

Miss T. leaped from a dearborn when the horses were in full speed, and lighting on one foot the malleolus externus was fractured, and the whole lateral portion of the capsule of the joint together with the lateral ligaments were ruptured. The joint was so much swelled in half an hour that it became necessary to cut away the stocking, and before the bandage was applied, which did not exceed an hour, the ankle was swelled to three times its usual size. The application of the bandage was made with as much force as could be, with a piece of common calico from two to three inches wide. Not more than four hours elapsed until the dressings became loose from the absorption of effused fluid, and for three days in succession it became necessary to reapply the bandage every twelve hours. Scarcely any swelling remained after the fifth day. Care was then taken to adjust the fractured bone and by a renewal of the dressings every four or five days the patient was quite well by the twenty-fifth day from the accident. Her sufferings were excruciating for the first two days, but before the swelling from effusion had subsided she became perfectly easy and had but little pain after. Except for the agency of the bandage, these two cases would have been tedious, painful, and hazardous to the joint, even had they not brought about a loss of the limb.

A servant in the employ of Col. T. dislocated his wrist. The bones were replaced by a Physician who gave orders to use cold water with a view to suppress swelling and inflammation. On the 10th day after the accident occurred I was called to see

the patient, when I learned that he had pumped cold water upon it frequently each day since the injury was received. The hand and fore arm were very tumid and painful and on the second day an abscess was opened extending from the hand to the elbow. By means of a properly adjusted bandage all suppuration was suppressed and the patient was well in four or five days. There can be but little doubt that the unpleasant symptoms in this case proceeded from the injudicious use of cold water, nor can the efficacy of the bandage in checking them and in restoring the patient speedily to health be questioned.

A little boy riding on the bannisters in the second story of his father's dwelling, lost his hold and fell through to the first floor upon his hands and head, whereby both wrists were dislocated, the external lateral ligaments of each being ruptured. The hands inclined to the ulnar side of the fore arm. Considerable effusion ensued before the dressings were applied, in consequence of which a reapplication of them was frequently called for during the first four days, until all the effusion disappeared. Care was necessary to prevent a morbid inclination of the hand and to facilitate a reunion of the external lateral ligaments. This was effected by means of a small splint, and at the expiration of the twelfth day the boy was quite well.

In all contused and lacerated wounds, as well those where the violence done is confined to the interior organization, as in others wherein the integuments are involved, the efficacy of the bandage is equally conspicuous. It must be admitted that in injuries of the kind in question, where the capacity for reaction is not destroyed, the danger in the case consists in the subsequent inflammation. Constant observation among those extensively engaged in practice, goes to prove the danger to be apprehended from mortification where inflammation runs high in contused and lacerated wounds. Any measure then which is calculated to secure parts thus injured, against engorgement and swelling accompanied by inflammation, must be considered important.

The advocate of the bandage entertains no doubt about the termination of any case, however extensive the injury, wherein

the recuperative powers of nature are not destroyed, provided pressure can be properly adjusted to the parts. Mutilations of the most shocking character, contusions and lacerations that invade extensively the natural structure of parts, have been known to heal as kindly, and to excite as little commotion in the system under the influence of a well adjusted bandage, as although the injury was a simple incision made by a clean cutting instrument.

The influence of pressure is not less decisively beneficial among those cases wherein all reaction is destroyed, since among these it tends immediately to excite the process of disjunctive absorption, while it prevents all tumefaction, inflammation, and deep seated abscesses.

A little girl twelve years of age playing in a horse-mill was caught by the knee between the principal wheel and the walls of the building, and was carried round to a point where the approximation was so close as to stop the machinery propelled by four horses. After remaining between five and ten minutes in this situation, by the aid of levers and wedges the limb was disengaged. On examination a few minutes after, I found all the integument, cellular substance, and most of the fascia protecting the muscles and tendons in the ham, for eight inches up and down the limb, were removed, and for about four inches around. The tendons of the biceps, semitendinosus and semimembranosus were exposed for at least four inches, and the head and upper portion of the gastrocnemius to a still greater distance; most of the bursæ being scraped off, thus leaving the tendons ragged and bare. The head of the fibula was completely denuded. A small portion of integument, rolled up like a cord, was left on the outer portion of this extensive wound. On the inner and front portion of the limb, and a little above the knee, a spot of some magnitude of a livid appearance indicated the opposite seat of pressure and violence.

The bandage was immediately applied from the toes to the hip, for the purpose of preventing all swelling and inflammation and to keep the limb extended during the cure. When the

dressing was applied the limb was nearly divested for the time of sensibility.

On the fifth day from the accident the dressings were removed for the first time, when the integument that was killed all came away, leaving a suppurating surface without either inflammation or swelling. On the twelfth day, and third dressing, by the process of sloughing, the whole of the lacerated fascia came away and left the entire surface of the wound healthy and suppurating. The flexor tendons were so extensively exposed that fears were entertained that they might slough, especially that of the biceps, the whole of which was exposed. All fears on that head were speedily dissipated by the appearance of granulations on all the tendinous surface. But from causes not known a fever now supervened, which was only subdued after a patient and unremitting effort of fifteen days. During the continuance of the fever, the patient lost as much by ulceration as the sore had gained previously by the granulating process, so that at the expiration of the first four weeks the wound was as large as at first, with a most obstinate disposition to contraction in the flexor muscles, created by some inflammation which had taken place. For it must be understood that from the loss of soft parts, the irregular conformation of the joint, and the impossibility of making complete, equable, and efficient pressure upon all points, the disposition to inflammatory action could not be entirely controlled. Under the persevering use of the bandage for another month however, no medicated application being made to the wound, she was so far restored as to render farther attention on my part unnecessary. The occasional use of a splint in aid of the bandage, was resorted to in order to counteract the flexor muscles of the limb.

Great apprehensions had been entertained for the preservation of the joint, because of the inflammation and the protracted nature of the case; but before the expiration of the third month, she was enabled to walk the streets with but partial stiffness in the joint, and that was temporary in character.

The frightful extent of the wound, the great loss of soft

parts, and the contusion resulting from pressure that suspended for a number of minutes the motion of machinery drawn by four horses, furnished ample apology for the amputation of the limb; nor is it probable that it could have been preserved by any other treatment than that adopted in the case.

The efficacy of pressure in cutaneous inflammation does not rest on my authority, since not only in our own country but in Europe ample evidence may be had of its beneficial effects. Who does not know that a scarlet state of the skin from inflammation, may be made to assume a white although transitory appearance by the momentary pressure of the finger? and who can doubt the efficacy of such pressure in disgorging the cutaneous bloodvessels, thereby relieving them of inflammation, when it is equably and perseveringly applied over the whole surface inflamed? The practice is as rational as it is successful. Whether the inflammation is of the erysipelatous kind, the result of varicose veins, or of deep seated inflammation progressing to the surface, the effects of pressure in preventing engorgement and suppressing inflammation are equally certain. Ulcerative absorption is the consequence of its influence upon those parts when nature cannot be relieved by checking the circulation.

As every one is prepared to comprehend, there is a wide difference among the various inflammatory affections of the skin; and notwithstanding pressure is beneficial in all, yet it is more especially among those of local origin, where its effects are more immediately and thoroughly decisive. Erysipelatous inflammation proceeding from constitutional causes cannot be rationally treated by any local remedy, since it is clear that the higher duty of the practitioner consists in removing the constitutional causes productive of the malady; yet even in this form of cutaneous inflammation, the effects of the bandage have been observed and recorded with approbation, by distinguished practitioners abroad and in our own country.

Inflammation of the capillaries, the absorbents, the veins, the arteries, nerves, ligaments, and capsules is of sufficiently ordinary occurrence to invite the attention of the practitioner to mechanical pressure with a view to cure.

The absorbents are a singular set of vessels, since they possess the faculty of extending inflammation as well towards the superficies and the extremities, as to the seat of circulation. The progressive inflammation of an absorbent vessel, whether produced by the application of a poison, the pressure of a morbid secretion, or by an irritating wound is entirely under the control of pressure; and where pressure can be efficiently made the effects are as speedy as they are manifest. It is upon this principle that a series of interesting experiments have been recently performed by the introduction of poisons beneath the surface by a gentleman of Philadelphia. Nor is it less singular than true, that while pressure will suspend entirely for the time the action of any poison upon this sub-system, still it has a most decided and powerful tendency in exciting the absorbents to act upon the natural structure of a part and thereby diminish its volume.

I have observed a bundle of absorbents as thick as a man's thumb at the end of the stump after the amputation of a leg for mammary sarcoma, in which the foot had been destroyed by the disease; and yet after a single application of the bandage, which remained on six days, not a vestige of these vessels could be seen. In that case while the stump healed in the ordinary time under the dressings by bandage, since it was well in sixteen or eighteen days, the absorbents and glands of the groin became affected by the same malady and soon after destroyed the patient.

The influence of pressure upon the healthy and morbid condition of the blood-vessels is by no means equivocal. Patients are occasionally known to die of inflammation of the arteries after the amputation of a limb, and there are many cases recorded of death from the inflammation of the venous system excited by the application of ligatures. Although the symptoms indicative of these forms of disease are more or less observable, the remedy is not of the doubtful class. The apposition of the inflamed surfaces of the vessel by means of pressure, is calculated to obliterate the canal and arrest the progress of the disease. This view of the subject appears to have been familiar to Mr. Hunter. But while the bandage may with great pro-

priety be urged in preference to all other treatment in inflammation of the blood-vessels, especial care should be exercised in discriminating the diseases to which it is appropriate.

Some modern pathologists have seen coagulable lymph in the femoral vein of the patient who has died with phlegmasia dolens and have hence concluded that this malady has its seat in the lining membrane of the veins of the limb; thus mistaking an incidental symptom for the malady itself. With these views before the practitioner, the patient would be thrown into hazard by graduated compresses placed in the groin for the purpose of obliterating the vein by pressure. Such treatment would certainly be unfavourable to the patient's safety, whose case would call for regular and equable pressure upon the entire limb. The lymphatic and glandular systems appear to constitute the seat of this malady, while it is curable by the bandage and thorough evacuations from the bowels.

The influence of mechanical pressure upon the arterial and venous systems demands the greatest attention from all those who pursue the practice, especially as the opinions of authors are, as may be seen by consulting Mr. John Bell, so vague and unsatisfactory on the subject. While the bandage exerts such a decisive influence upon the absorbents under all circumstances, there are conditions of the blood-vessels where it has a manifest tendency to excite preternatural action.

That the absorbents are excited to inordinate action at the same time that the bandage impedes arterial circulation, is a principle of very important practical application: and that partial pressure should under any circumstances become an exciting cause of arterial action is a fact not less interesting than true.

Those wounds in which small blood-vessels have been divided, should not be dressed until time is afforded for reaction; and in every instance in which the dressings are applied without securing all the vessels by ligature, they should be put on so loosely as not to control the divided surfaces; or with a sufficiency of force to command the arterial action of the part. Without this precaution, a tendency to hæmorrhage is excited by the

dressings, and blood continues to flow until the stimulus of pressure is removed. Partial pressure would thus destroy a patient whose case might otherwise have been innocent. The remarkable case of a wound of the principal gluteal artery, wherein the patient bled nearly to death, and in which the blood was confined beneath the muscles of the part, together with the irrational practice of treating uterine hæmorrhage by the "*tampon*," are in point. The yielding nature of the muscles in the one case, and the expanding walls of the uterus in the other, become aggravating causes of the hæmorrhagic state of the bleeding vessels. No practice is attended by more hazard, and none more untenable than that of treating uterine hæmorrhage by the "tampon."

The hæmorrhagic excitement which I have so often witnessed as a consequence of dressings upon wounds, and which as often subsides upon their removal, is not more demonstrative of the injurious tendency of partial pressure, than of the mischievious influence of the "*tampon*" in uterine hæmorrhage.

But while decisive pressure is preferable to all other modes of stopping hæmorrhage, the varying conditions of many cases renders it necessary to be watchful of its effects. Drs. Gaither and Winston have recently published a case in this journal in point. The bandage, was applied to a wounded ulnar artery near the elbow, wherein swelling, inflammation and sloughing had supervened. The effect of the dressing was that of checking hæmorrhage immediately. But so soon as by absorption the bandage became loose the bleeding recurred. I have had many cases of a similar character.

Larry and Abernethey both speak of the influence of pressure in checking hæmorrhage, the one in allusion to the axillary, the other to the carotid artery.

I have succeeded in controlling hæmorrhage from the femoral artery the result of gun-shot, and from the humoral produced by a knife wound, by means of the bandage. Drs. Venable and Turner have lately had a case of gun-shot wherein the axillary artery was opened in which the wound healed by the first intention under the influence of the bandage, as recently

reported in this journal. In most cases of wounded arteries of the head, hand, fore-arm, foot and leg, compresses and bandage are alone sufficient to place the patient in safety. There may be cases, however, even when the smaller arteries of the limbs are opened, wherein the ligature is preferable to mechanical compression. If such do exist, they are to be found among those cases in which after long, varied, and unsuccessful efforts, the limb becomes swelled and the wound is in a sloughing state. Mechanical pressure operating in a case like this, as well upon the accompanying nerve and vein as upon the artery, might possibly aggravate the disposition to slough. The case however already alluded to of Drs. Gaither and Winston will have its effect in settling this particular point in practice.

A servant had the brachial artery opened by an ignorant bleeder, and after some trouble the loss of blood was checked. In a short time, however, the entire fore-arm and arm swelled to a very great extent from an effusion of blood into the cellular substance, and when he became my patient, being ten days after the accident, the limb was highly inflamed and excessively painful. Bandages were applied to the fingers, hand, fore-arm, and arm, and removed as often as they became loose from the reduction of the size of the limb by absorption. At the end of eight days there was neither swelling nor soreness remaining. A scab of considerable magnitude occupied the site of the original wound in the integuments, while there was evidently a close adhesion between the wounded vessel and the superficies. Difficulty was now anticipated in the subsequent treatment of the case, because of the uncertainty of any efforts in making equable and efficient compression on a surface so illy adapted as that of the bend of the elbow. For a number of days the issue of the case remained doubtful, notwithstanding the utmost care in the application of graduated compresses secured by the roller. Finally it became evident that an eschar would separate, as suppuration was manifest around its edges. For three days in succession this event was looked for. At last on removing the compresses the eschar came away and thus opened the artery. A ligature was applied to the vessel about three inches

above the elbow. No further trouble was experienced in the case and the patient was quite well in a week after.

A man divided the arcus palmaris near the middle of the palm of the hand, and under the influence of various contrivances it continued occasionally to bleed until the conclusion of the third week, when I was called to see him. The tourniquet applied above the elbow was nearly buried in the swelling of the limb. The wound of the hand was in a sloughing condition, while he had enjoyed scarcely any repose since he met with the accident. Upon removing the cloths and dead flesh from the hand, the hæmorrhage recurred, and finding by pressure that the blood was furnished by the radial artery, that vessel was immediately secured by means of a ligature. In a few days the hand was restored to health.

A cooper divided the tendo Achillis and posterior tibial artery by stepping backwards against his axe. The hæmorrhage was profuse, and the artery being involved in a large amount of cellular substance, retracted so far that its extremity could not be seen. Under these circumstances it was determined to restrain the hæmorrhage by pressure. For this purpose, as well as with a view to reunion of the divided tendon, a bandage was applied upon the toes and foot, and another from the knee to the ankle, care being taken to press the muscles before the dressing, in order the better thereby to approximate the divided parts. When by the aid of the bandage, with a favourable position, the divided surfaces were all perfectly apposed, pledgets were laid in the direction of the artery, and secured by the roller. With the aid of two dressings the tendon was reunited, and the vessel obliterated at the expiration of the second week.

Wounds of the radial, ulnar, and interosseous arteries, those distributed upon the hand and fingers, the tibial, peroneal and plantar arteries, together with those on the scalp, have all been treated successfully by the bandage; nor have I ever encountered the least difficulty in any similar case. The bandage is calculated to put an immediate check to the loss of blood, and in comparison with all other remedies, it is easy of execution,

free from pain, and not calculated to excite the fears or apprehensions of the patient.

Most practitioners of some experience have witnessed hæmorrhage from the venous system in cases of varicose swelling, and during the period of utero-gestation. Among females who are not the subjects of varicose swellings, I have seen the veins of the leg with the integuments open and bleed freely.

In all similar cases, as well as those proceeding from a varicose condition, pressure is the exclusive remedy. The Faculty generally concur in the use of pressure to stop venous hæmorrhage, and it is matter of great surprise that so little regard should be paid to it when the artery is the source of the blood.

The French and Italian authors have very generally kept the subject of mechanical pressure before the public as an important agent in the treatment of aneurismal affections, notwithstanding other writers of distinguished standing have inculcated the propriety of relying alone upon tying the diseased vessel.

It is now a long time since Mr. Hunter explained the advantages of applying the ligature as distant from the seat of aneurism as possible in order to embrace a portion of the vessel that is capable of healthy union. Many surgeons have urged the propriety of delaying the operation as long as consistent with safety, in order to afford the small anastomosing vessels time to enlarge, sufficiently to carry on healthy circulation when that in the aneurismal trunk is cut off by the ligature. Since mortification of the limb after tying the vessel has occurred sufficiently often to attract particular attention to this circumstance, it has been proposed by a distinguished surgeon, Monsieur Dubois, to apply several ligatures on the same vessel at certain distances from each other; the one nearest the source of circulation to be so lightly drawn as to offer but little resistance to the blood, while the remaining were to be applied with more force but not sufficient to close the vessel. A mechanical obstruction thus set up, it was thought, must have an effect in enlarging the anastomosing vessels and thereby the danger from mortification would be lessened, when the vessel became finally closed by drawing upon the ligatures. Various kinds of ligatures have

also been urged upon the attention of professional men, while the practice of applying two and dividing the vessels between them has been advocated by the most distinguished surgeons. The animal ligature has been introduced by distinguished authority and must combine advantages not to be secured by those in ordinary use when the wound is dressed in the ordinary way; but where the bandage is used in cases of wounds of the blood-vessels, it becomes matter of great indifference what species of ligature is used. The animal or the silken, the large or the small ligature, whether drawn with great force or barely sufficient to arrest the current of blood, may be indifferently selected; nor is it possible after the dressing by bandage is applied, that any unpleasant consequences can ensue either from the kind of ligature, or the mode of securing it on the vessel. The bandage takes from the one all its superiority, and from the other all its defects.

I have used the bandage with entire and very speedy success in the cure of aneurism upon the scalp, the face, the arm, the fore-arm, the hand, the foot, and the leg. Aneurismal swellings upon the scalp, however extensive, and from whatever source supplied with blood are perfectly manageable by the aid of a common roller and compresses; nor do they require the partial operation suggested by Sir A. Cooper of dividing the principal arteries running to the swelling, since these can be as effectually put under the influence of compression as the swelling itself. Cases of the kind in question occupying one half of one side of the head and forehead, involving all the upper eye-lid, the inner as well as the outer canthus of the eye, have been speedily cured by mechanical compression.

A youth engaged in hunting accidentally opened the left brachial artery with his knife. After some trouble his associates succeeded in checking the hæmorrhage, but in a short time an aneurismal tumour presented itself at the site of the wound. Although the cure was tedious by reason of want of skill in applying the dressings, yet it was ultimately complete, no other mean than those of roller and compress being used.

A gentleman sustained a compound fracture of one leg, and

on the removal of the dressing five days after the accident, an aneurismal tumour of considerable size was manifest, being the consequence of violence done the anterior tibial artery. Compresses of proper thickness and magnitude were placed directly on the swelling and retained there by means of the only dressing of the limb, a common roller reaching from the toes to the knee. In two dressings this tumour disappeared, and before the expiration of the fourth week the gentleman was upon the streets.

A young man came before the medical class with aneurism of the radial artery the consequence of a wound. The entire limb was excessively swelled and very painful. Pulsation was remarkably high and strong in the tumour. A bandage was immediately applied reaching from the hand to the shoulder, and because of the rapid reduction of the swelling, the bandage was taken off and renewed daily. At the expiration of a week, he again came before the class; when by admeasurement the aneurismal limb had been so much reduced in magnitude that it proved to be much smaller than the other. Considering himself well, sufficient precautions were not taken in the subsequent dressing; in consequence of which after a short time a small anuerismal tumour showed itself about two inches below the other, occupying as was supposed a portion of the sack of the previous swelling which had not been obliterated by pressure. Graduated compresses and the roller applied with care reduced this swelling in a few days, and thus rendered the cure complete. Experience has taught me the propriety of continuing the treatment for some little time after all swelling and appearance of disease has vanished, to give support to the vessel until it regains its natural tone.

In the fall of 1825 I operated on an old gentleman of sixty five years of age for axillary aneurism. A knowledge of the difficulty to be encountered in this operation and of the frequent failures of the most distinguished surgeons in attempts to take up the subclavian, excited great solicitude; but inasmuch as the tumour appeared to be larger than a quart pitcher, and pulsated violently upon all points of its surface, while there were

fears entertained that the integuments would speedily ulcerate and the patient bleed to death, it was determined to attempt the operation without much delay. The patient being placed half reclining, his head inclined to the sound side and the shoulder depressed as much as possible, presented the parts in a state most favourable for the purpose in view. Several vessels, some of magnitude, were exposed and held out of the way, by which means the sub-clavian was brought into view and the ligature applied without the loss of a table-spoonful of blood. By the twenty-first day from the operation the patient left town in good health, free from all pain, and relieved of the distressing consequences of taking daily large quantities of laudanum to procure ease. The needle with which the ligature was passed around the vessel had been constructed for the purpose of aiding in the removal of guttural polypi, and need not be described, since no surgeon should attempt an operation who is unable to adapt his instruments to the case before him. Immediately on the application of the ligature, the members of the faculty and pupils present on the occasion, examined and could discover no pulsation in the tumour or vessels of the arm, and yet there was no loss of temperature, although a considerable defect of sensibility for some days ensued. Whether the most skilful application of pressure would have succeeded in the earlier stages of this case cannot be known. My knowledge however of its influence in a variety of cases prepares me to anticipate a great extension of its usefulness in the hands of the surgeon, and I should in the earlier stages of all aneurismal affections of the thigh and arm place great reliance on the corrective influence of pressure judiciously applied.

Of the influence of pressure on the secretory and ulcerative processes. Secretion and ulceration are in a particular manner subject to the influence of the bandage, and it is by reason of its controlling power on these two functions in the animal economy, that the bandage becomes a remedy of inestimable value in the treatment of ulcers. Pressure is not less decisive in promoting purulent secretion upon denuded surfaces invaded by high degrees of inflammation, than in checking those pro-

fuse secretions which are a consequence of an exhausted state of the strength and energy of the system. While intermitting pressure is daily seen to produce a thickened state of the integument, as on the hands of the labourer, and the feet of those who go without shoes in the warm season of the year, it as uniformly excites the absorbents, attenuates and even destroys the same surface, when made with more uniformity or with more intensity. The paper already published in this Journal on the influence of the bandage in the cure of ulcers, renders it unnecessary to say more in this place on the secretory and ulcerative processes.

Phlegmasia Dolens.—The pathology of this disease appears to me to be very defective, and I am inclined to believe that the treatment commonly recommended, and especially that which must be adopted under the idea that it proceeds from inflammation of the veins of the limb, is exceedingly incorrect and calculated to be very mischievous in its consequences. Although the precise causes of the malady are not understood, yet no one can look upon a case of phlegmasia dolens and fail to observe how deeply the cellular substance and glandular system are involved, even if they do not constitute the exclusive seats of the malady. Recent post mortem examinations showing an inflamed condition of the great femoral vein, are calculated to betray the practitioner into erroneous theories and mischieveous practice. Insulated pressure upon the upper extremity of the vessel, by obstructing lymphatic circulation and aggravating glandular congestion would certainly aggravate the disease. The common roller kept closely drawn from the toes to the hips, aided by the active and repeated use of cathartics, succeed much more certainly and speedily in affording relief than any treatment known beside.

Many cases of phlegmasia dolens terminate in a permanent enlargement of the limb, with chronic inflammation and repeated ulcers about the ankles and posterior part of the leg. A case of this description has been under my charge during the present season. A lady was afflicted with phlegmasia dolens twenty years ago after her first confinement, and with each

birth since that period, some ten or twelve in number, she has suffered more or less with inflammation and ulceration of the leg and ankle, while at no time during this long period has she been free from an indurated condition of the cellular texture of the limb, especially on the back portion of the ankle and leg, accompanied by soreness always troublesome, and very often so great as to confine her for weeks in succession to her chair and bed. Her general health has always been fluctuating; sometimes for many months in succession she has been the subject of active disease and of great emaciation, while on other occasions she is puffed up with the flattering manifestations of health and vigor. Only a few months ago she became my patient. The limb was much swelled, and indurated, with a large ulcer just behind the outer ankle and another a little below the thick part of the leg. The leg was inflamed and very tender to the touch. A bandage was applied with caution from the toes to the knee. This was renewed every other day, while no application was made to the ulcers, care only being taken to subdue their morbid sensibility by pressure with the hand for a few minutes before the bandage was put on. By the expiration of the fifth week all the induration, enlargement, inflammation and soreness were relieved and the ulcers healed; while the limb is smaller and healthier at this moment than at any previous period for almost a quarter of a century. The derangement of the digestive organs made it necessary to use cathartic medicines every other night for three months in succession. She is now in the enjoyment of good health.

Bandage in Paronychia.—Were the bandage productive of mischief by disposing parts to mortification, there would probably be no set of cases within the range of surgical practice wherein such an occurrence should so readily be expected, as in inflammations of the thumb and fingers in the shape of that very common and painful affection Paronychia. On the thumb and fingers we find no protection given to the blood-vessels against the consequence of the direct influence of the bandage; and yet neither in a diseased nor sound finger can a bandage of calico, the article always selected for bandages, be applied

with such force as in any degree to hazard the circulation of the part. Equable pressure there as upon all other parts has an immediate effect upon the absorbents; and the diminution of magnitude by reason of absorption must correspond with the force used in applying the roller. The only solicitude which the surgeon can rationally entertain in regard to the consequences of pressure, consists in the uniformity with which it is applied: and yet when we see the tourniquet buried almost in the arm in order to check hemorrhage and continued with occasional intermissions for three weeks in succession, without destroying the limb, what rational fear can be entertained of the influence of the bandage, when by its equal action upon all parts swelling and obstruction are prevented?

This painful inflammatory affection is of three different species. One is purely cutaneous and is much inclined by reason of its situation at the root of the nails, to pursue its ordinary course to suppuration, because of the difficulty of making pressure at that point sufficiently regular to suppress all inflammation.

When the disease originates beneath the fascia that binds down the tendons of the different muscles inserted into the phalanges of the fingers, it is characterized by great swelling and inflammation not only of the finger affected, but of the hand and occasionally of the fore-arm also. Suppuration never extends beyond the immediate vicinity of the root of the nail in cutaneous paronychia, while the injury done, and the loss sustained involves only the nail; whereas when the disease is seated beneath the tendinous fascia of the finger, the source of purulent secretion is extensive, and the pus makes its way through the finger, the hand, the wrist, and fore arm, and occasionally as high up as the elbow. There is probably not less difference in the appearance and extent of these species of paronychia, than in the destructive consequences of each. The use of the hand is sometimes seriously and permanently injured when the disease is seated beneath the fascia, because of the extent of inflammation and suppuration.

But it is in that species yet to be named wherein the suffer-

ings are most intense. A small secretion not much larger in amount than a grain of wheat, first takes place on the surface of the bone which destroys the connexion between the two, excites thereby a high degree of inflammation in the periosteum and causes an extensive separation between it and the bone, in consequence of which the latter dies. The patient is in agony for several days and nights with a pain which he can refer to a point, while no apparent manifestation of disease exists. The first phalanx of the thumb or finger is always the seat of this form of the malady. The part does not begin to swell and give external evidences of inflammation until all the mischief, which consists in the death of the bone, has been accomplished. The separation and removal of the dead bone is always a tedious and extremely painful process.

The bandage secures higher advantages than any other remedy in each variety of this disease: applied in the early stage, it cannot fail in competent hands to put an immediate check to the two worst species of paronychia, and thus restore the parts to health in a few days. It might be supposed that a part already in a high state of inflammation and excessively painful, would give the patient so much agony under the influence of the bandage that he would not be able to bear it. In the most torturing case however that I have ever seen, the part could be gently and progressively compressed in the hand with a rapid and extraordinary diminution of sensibility and pain. In five minutes I have often succeeded in applying all the force of my hands to a part thus situated, without aggravating the sufferings of the patient. He would then be prepared for the application of the bandage. Without prompt and efficient measures the bone is necessarily lost in the worst species of paronychia, but even then we should not cease our efforts to preserve the length of the finger, of the practicability of which satisfactory evidence is adduced in the following cases.

A youth in a cotton factory of this place, was attacked with paronychia in the right thumb of that species involving the bone. Before I saw him suppuration had taken place, the matter discharged by two orifices and the thumb was very much swelled.

Without delay I proceeded to extract the bone which was accomplished with but little diffiiculty by uniting the two orifices. After the bone was removed the thumb was pressed some time in my hand, using very gentle pressure at first. Then by the application of a bandage its transverse diameter was diminished and the thumb elongated, so that when all internal suppuration ceased it was something like half an inch too long. In a few weeks a new bone was secreted and the thumb is now as strong as ever, but somewhat inconvenient by reason of its length.

A lady of this town wounded the fore finger with a bone of a fish, and six weeks after I removed the first phalanx, it being dead, while the soft parts were preserved. By means of the bandage the soft parts were prevented from contracting, a new bone has been deposited in place of that removed, and the finger is now as long and as useful as before the accident happened.

A servant woman came to me with paronychia involving the first joint of the thumb, the bone of which was already dead. The part was very much swelled, exceedingly painful, and suppurating. Before she was permitted to leave me, the bone was exsected and the thumb bandaged. In a few weeks it was restored to health and of the proper length. In consequence of the influence of the bandage the nail was a little deformed. There was no other perceptible difference between it and the other thumb.

There can be no question of the superior virtues of the bandage even to the knife in the early stages of this disease, since it is better calculated to subdue inflammation than any other remedy known.

Phymosis and Paraphymosis.

Sir Astley Cooper has written a highly interesting and copious article on these two subjects, in which is condensed a large amount of practical matter; and since these diseases are frequently destructive of the glans, prepuce, urethra, and even the body of the organ, a correct and familiar acquaintance with what is published on the subject, is peculiarly necessary to the practitioner, who is often called on to witness cases of

great and protracted agony with extensive mutilations under the best treatment inculcated in the books.

If a new remedy and one calculated to afford immediate relief can be suggested to the medical community, a remedy that secures ease in a shorter space of time than a dose of laudanum would stop a cramp, such a remedy must command the support and approbation of the whole medical community.

Dr. —— came to town and brought with him a little boy who had been for ten days the subject of phymosis. The magnitude of the parts, the colour of the glans, the protracted duration of the case, together with its having resisted all the usual applications, induced a belief that part if not the whole of the glans would mortify, were it not already dead. After the examination was completed, one of my pupils was directed to take the whole swelling in one hand and to press gently upon it with the other, taking care with the progressive reduction of the swelling to press his hands regularly together. In less than five minutes the glans became soft and flaccid, the patient complained of being faint and immediately fell senseless on the floor. In this situation there was no obstacle to covering the glans with the prepuce, so that in ten minutes the patient was prepared to go home entirely relieved.

A young gentleman whose prepuce was very luxuriant, exposed the glans for the purpose of curing a slight ulceration occasioned by the natural secretions retained on this part too long. With a view to make his applications to the glans it was kept unprotected by the prepuce until phymosis ensued. Being somewhat acquainted with medicine various means were resorted to for several days in succession, with the consequence of increasing pain, swelling and inflammation. Losing confidence in his own skill, a messenger was sent for me. I found the part excessively swelled, morbidly sensitive to the lightest touch, and constantly very painful. One of his room mates was directed to apply gentle and regular pressure by taking the parts between his two hands. In a few minutes the swelling was reduced, the prepuce replaced upon the glans, and the patient to his astonishment entirely relieved.

A young man the subject of syphilis, had several chancres on the glans penis that gave him some pain and excited great apprehensions that they might destroy this part. With a view to arrest their progress and to heal them if possible by local remedies, the glans was kept uncovered and caustic applications made to the ulcers. Under this treatment severe phymosis ensued, and the patient became alarmed from the danger of mortification of the whole glans, even should the part escape injury from the chancres now more threatening in their aspect than before.

Thus situated he became my patient. By means of pressure between the palms of the hands, adapted to the regular and decreasing magnitude of the part, I was enabled to draw the prepuce upon the glans in less than ten minutes. The parts were excessively sensitive to the first touch, yet this morbid sensibility subsided in less than two minutes, while he was entirely free from all pain so soon as the prepuce was replaced upon the glans.

Were it necessary I could swell this article with a history of twenty cases which have been cured in a few minutes; while in several of them, all the usual remedies had been unavailingly used from five to fifteen days. The philosophy of the treatment is as plain and intelligible, as the application is simple and decisive. When such pressure is put upon the glans as to arrest arterial action, it is clear the veins and absorbents must empty the part of all fluid in a few minutes and thus relieve it of stricture.

The application of the hands in these cases is greatly preferable to pressure by means of the roller; yet in all cases where the object is to be accomplished slowly, as in the removal of organized substance, the bandage must be relied on.

Probably no set of cases in the whole range of surgery excite more solicitude; and are attended by more painful mutilations, than those wherein paraphymosis is a characteristic feature. All obstinate venereal sores proceeding from constitutional causes, or from the contact of local poisons, as well as those proceeding from the multiform instances of syphilitic chancre,

are strongly inclined to produce, or to be attended by paraphymosis. The ulcer upon the glans inflames and is painful; the part swells and with the discharge of morbid secretion the ulcer extends sometimes with great rapidity, to the destruction of a part or the whole of the glans, and the external end of the urethra, and even the cavernous bodies themselves are destroyed in some instances. With the swelling of the glans the opening of the prepuce becomes contracted, so as to exclude the ulcers from view: hence the tedious nature of these cases, the ravages committed by ulceration, and the painful operations that are too frequently performed on the prepuce.

The single consideration that high morbid action often goes on unchecked by the operation of any of the accustomed remedies in these cases, suggests the importance of resorting to other treatment which promises a fairer prospect of success. While there is a considerable variety in the character of those ulcers that attack the glans and prepuce, experience does not warrant the conclusion that they require a corresponding variety in the treatment. A diversity of causes frequently produce the same effect on the system. A painful inflamed ulcer may be of syphilitic origin, or the result of morbid causes purely local in their operations; they may proceed from pseudo-syphilis; or from mercurial irritation. The ulcer enlarges through the active agency of the absorbents and these are stimulated to action by various causes.

One object then of high import presents itself to the surgeon as worthy of peculiar attention. Inflammation, pain and swelling, attendants upon ulceration of the glans, whatever the cause producing them, demand much of our care, and the local remedy best adapted to the correction of them is that upon which too much time cannot be bestowed.

It is true that ulcers of a specific kind cannot be divested of their peculiarity by any local treatment; yet inasmuch as specific ulcers commit ravages only so far as they are attended by an inflamed, swollen, and painful condition of the parts, any remedy calculated to alleviate, check or arrest these symptoms must be of inappreciable value.

A young man the subject of great chylopoietic derangement was attacked with ulcers on the glans penis. Being under the influence of some fever daily, and of great torpor of his bowels, unusually large doses of medicine became necessary in order to produce any sensible effect. The ulcers soon spread extensively over the glans attended by great pain, swelling and inflammation of the part. The prepuce which at first could be retracted with a view to examine the ulcers now became inflamed and severe paraphymosis ensued. For six weeks this patient suffered severe and constant pain in the seat of the ulceration. Finally his loud complaints and sleepless nights attracted the attention and alarmed the apprehensions of the family and neighbours. I was now called to see him and found the prepuce and glans deeply involved in ulceration although it was impossible to see the extent of it. Pressure was immediately made upon the part by means of the hands, which was continued for probably ten minutes in order to subdue morbid sensibility. A bandage was then closely applied including the extremity and body of the organ. In less than fifteen minutes after the dressing was put on he expressed himself as relieved of all pain, slept the entire night, and up to the period of his restoration to health he complained of no farther pain in the ulcers. The bandage was continued as well with a view to prevent the recurrence of inflammation and ulceration as to favour the process of cicatrization. Under the influence of purgative medicine with a view to correct the diseased state of the digestive organs this patient was restored to health in a few weeks.

A servant boy who was the subject of a severe attack of syphilis was put upon the active use of mercurial medicine with a view to cure. Several chancres upon the glans, of some magnitude, were observed on retracting the prepuce. Before the medicine had time to produce any effect, the swelling and inflammation of the glans became so great that a farther examination was impracticable. The increasing swelling and inflammation, with an ichorous secretion from beneath the prepuce excited solicitude about the safety of the glans. This circum-

stance induced the practitioner to resort to a wash of corrosive sublimate dissolved in spirit which increased the distress of the patient. Finally by advice pressure was made to the parts. In a few minutes by means of the hands the sensibility of the swelling was principally removed, and then by the subsequent use of the bandage the ulcers healed without trouble or difficulty.

By the continued use of pressure I have not failed in relieving any of those cases of paraphymosis which are the consequences of recent inflammation. Neither in phymosis nor paraphymosis can I acknowledge any propriety in the customary operations performed on the prepuce. While an operation fills one indication, the effect of pressure is such that nothing is left to be done: while the one is remotely tributary, the other brings immediate relief. There is one species of paraphymosis to which the bandage is not applicable so far as regards the state of the prepuce itself. This is more generally congenital, yet it is sometimes acquired. When by time and tedious inflammation the internal fold of the prepuce is converted into a cartilaginous cap, there is no mode of treating it successfully but by exsection. I have known this state of the prepuce to be hereditary.

The bandage not only answers every indication in cases of phymosis and paraphymosis, but in troublesome ulcers upon any portion of the organ it is superior to every other remedy. A young gentleman travelling under peculiar circumstances had an ulcer on the body of the penis which, notwithstanding all the care he could give it, became of frightful magnitude and extended more than half around the organ. So soon as he made his situation known to me, a bandage was drawn closely upon the whole member so as to prevent erection. It had no doubt been prevented from healing by the variable condition of the circulation in the part, inasmuch as he recovered in a few days under the influence of the bandage. In regard to all ulcers upon the prepuce and glans, attended either by phymosis, or paraphymosis, whatever the extent of inflammation, and notwithstanding the variety in these affections, whether simple, or

of a specific nature, mechanical pressure is the only local remedy which can claim, or is entitled to unqualified confidence. Specific as well as simple ulcers are divested of inflammation, of pain, and of swelling by pressure; and however strong the tendency to progression may be, yet they cannot present strong morbid tendencies while under the influence of the bandage. The various poultices, &c. which we are taught to use in these cases, and under the influence of which are so often seen the most destructive mutilations, even to the extent of the whole of the external organs of generation, constitute a strong argument in favour of innovation. Notwithstanding the noxious atmosphere of the venereal wards of the large hospitals in the great cities of Europe, and to which is no doubt justly attributed the strong inclination to slough in syphilitic and other venereal ulcers of the genitals, yet it is believed that the bandage would preserve most of those patients from serious loss or injury.

When the tumid state of the part is subdued, the inflammation dispelled, and the morbid sensibility relieved, then all the local causes productive of sloughing are removed: and since the bandage exerts a most controlling power in all cases where it can be well applied, it would seem that confidence cannot be withheld from it by the thoughtful in the cases under consideration. The syphilitic patient in the venereal wards at Guy's and St. Thomas', cannot be in more danger of losing his genitals, by reason of confinement in an impure air, than of losing his life, provided pressure is skilfully applied to the ulcerated surface. The tendency of the bandage is to prevent any concentration of morbid action upon the parts to which it is applied.

Chorde.—This is a painful inflammatory affection, more distressing than dangerous, and for which a variety of remedies have been proposed by practitioners. Inasmuch as it is never troublesome except when the organ is erect, there is no symptom more under control, and none to be prevented with more facility. Pressure applied to such an extent as to prevent erection, must preserve the patient free from this most annoying symptom, while it is equally well calculated to subdue the disease of which the symptom is a characteristic. I have often

known it to prove a most tedious and annoying symptom in gonorrhœa, notwithstanding the use of the bath, laudanum, camphor, cicuta, and all the common remedies in such cases.

It is instantly checked by pressure.

The usual manner of dressing a stump after the amputation of a limb, will appear exceedingly defective when the superiority of the bandage becomes correctly known. Most surgeons have witnessed the occasional occurrence of fistulous abscesses in and near the stump, that have always proved tedious and too often hazardous in character. The tedious suppuration and protracted cure in these cases, are familiar to all. The occasional difficulty attendant upon ligatures has caused particular attention to be directed to this subject, and while one distinguished surgeon makes the addition of the animal ligature important in the treatment, another of no humble pretensions uses no ligature at all to the largest vessels; while a third, who considered the agency of the ligature indispensible, to render it more secure in its place, since he had seen it slip off with the loss of the patient, proposed the passage of its ends through the end of the vessel and then to secure it by a second knot. A variety in the shape and size of the ligature has furnished matter of warm contention, while the mode of applying it, and the manner by which the vessel is permanently closed, are also subjects of inquiry. Had those surgeons been made acquainted with the bandage, they would have recognized in its operation an agency so controlling on the blood-vessel, as well as on all the other parts involved in the wound, that they would have ceased to speculate about the size, shape, structure, and mode of applying ligatures; because of the safety of the patient who is put under the influence of a roller, and of the great facilities secured thereby in bringing about a more certain and speedy cure, notwithstanding any objectionable features in the ligature.

After amputations the bandage should be applied from the nearest joint above, down to and over the end of the stump. It is thus that all the muscles are brought down and made to protect the end of the bone, the lips of the wound are approxi-

mated, the divided blood-vessels sustained, the disposition to swelling and inflammation resisted, the formation of abscesses prevented, and the entire wound caused to heal in a great measure and sometimes entirely by the first intention. The use of adhesive strips of plaister, this valuable addition in the treatment of wounds, is often superseded by the proper adjustment of the roller upon the end of a stump.

Besides the superior consideration of greater safety to the patient, this mode of treating amputated limbs recommends itself most strongly to the attention of the faculty by reason of the remarkable curtailment in the surgeon's labour. While a perfectly quiet state of a wound is most conducive to the speedy acquisition of health in the part, the treatment by bandage will enable one surgeon to do the duty of half a dozen in the camp, in the hospital, or on board a man of war.

An apology for a frequent renewal of the dressings has been urged from considerations of cleanliness, and surely one of more magnitude can scarcely be imagined; and the bandage in this particular stands conspicuous over all other applications, since it is calculated to prevent those secretions which soil the dressings and bedding, at the same time that the actions in the wound are confined more especially to immediate reparation. But even where cleanliness would seem to urge the propriety of frequent dressing, the use of a bandage made of the proper material will admit of a sponge and warm water to the extent necessary for the removal of every thing offensive without disturbing the bandage, a desideratum in the treatment of wounds. Every one in any respect conversant with the principles of surgery, knows that the process of reparation is retarded by opening the wound and permitting its opposing surfaces to recede, whereby all recent formations are torn asunder. A bandage of thin muslin, the common calico of the stores, is the best material for all dressings, since it will permit morbid secretions to exude through its texture, while it is sufficiently strong for all the purposes of surgery.

Those made of thick muslin or of flannel are objectionable because they want pliancy and prevent transpiration. A thick

stiff bandage, like a shoe of stiff unyielding leather, presses irregularly, and excites pain and even ulceration; whereas one made of soft and thin material adapts itself somewhat to the variable surface and thus presses more equably.

ART. IV.—*Essay on the use of Cathartic Medicines in the Diseases of Women.* By John E. Cooke, M. D. &c.

It is a very prevalent opinion with the mass of the people and even with physicians, that it is unsafe to give cathartic medicines to women in a state of pregnancy, and even doubtful during their ordinary periodical menstruation. The consequence of this error is the omision, in very many instances, of the only medicines that can relieve the patient; and frequently death follows from the neglect. Of this the writer has witnessed or been acquainted with not a few cases. It is his purpose in the following pages to state his experience on this subject, without any account of the theory which led him some years ago to adopt the practice. He has in some previous papers, endeavoured to give an abstract of the theory of some diseases, as stated in his work on Pathology and Therapeutics; but is deterred from so doing for the future, because he finds that those who choose to make remarks, found them on what is stated in those papers, and not on the work itself. As the grounds on which the practice recommended in that work, are there stated in as condensed a manner as he is capable of, the attempt to give an abstract was not perhaps wise; and he may have to blame himself more than others for any defect they may think they see in the argument. Be that as it may, he will here confine himself to the practice. Those who feel inclined to have further information on the subject, can consult the work itself.

It may perhaps be doubted by some physicians who have but lately entered the practice, whether the apprehension of giving

cathartics to women in the above circumstances, is so great as has been represented. The writer however has frequently been asked, within the last ten years, by physicians, whether it was *safe* to give a cathartic to a pregnant woman. And not many years farther back, it was a common opinion, and is yet entertained by some, that a dose of calomel will produce abortion. About twelve years ago the writer knew an instance of a young physician being immediately dismissed from his attendance on a lady in the eighth month of pregnancy, for recommending ten grains of calomel for a pain in the bowels.

Many years ago the writer, under the influence of the general principle, *that patients in autumnal disorders recover when well evacuated from the liver, and die if they are not,* was in the habit of administering mercurial cathartics to pregnant women, as to all other persons, without regard to any other rule than that *of producing consistent bilious evacuations as soon as possible.* He succeeded as easily with them as with others, and is able to establish the fact by reference to books, that he was as successful in the treatment of pregnant women as of any other persons.

In the year 1818 he was called to see a lady in the ninth month of pregnancy, who was in such pain, recurring at intervals, that she was supposed to be in labour. The peculiar expression of the countenance when the pain was felt, the suddenness and violence with which it seized the patient, together with the fact that the os uteri was not at all dilated, although the patient had been in this situation two or three days, induced him to administer immediately a dose of calomel. The discharges produced were green, and for six hours after the operation there was no return of pain. On the first return, she took another dose, a very large one, thirty grains, as she was ill. This operated well and relieved most remarkably. As the discharges were still dark green, she took every day pills of scammony and calomel, enough to operate freely. In four or five days she was quite well and about the house. In a few days afterwards, she had an uncommonly easy labour of about an hour.

It is not however his object to dwell here on the propriety of

using cathartic medicines in such cases. No one will question the propriety of doing so, if it can be safely done; and that it can be, no one can well doubt after reading the treatment of the following cases, in which those remedies were successfully used to obviate the very evil which they have been supposed to produce.

Having been led by the theory alluded to above, fully to believe that cathartic medicines which evacuate from the liver, would effectually restrain menorrhagia, the practice was followed in the first case which presented. The following is an extract from the work on Pathology and Therapeutics above mentioned, on this subject.

"The first time this practice was employed was in 1822, in the case of a lady perhaps twenty-five years of age, naturally of a full habit, accustomed to living well and easy. She had been in bad health for some years, had had a violent bilious fever in the preceding autumn, was at the time mentioned thin and sallow, extremely weak, and scarcely able to walk steadily, and for a week or two had had a very great and continued menstrual discharge.

I gave her sixteen pills of scammony and rhubarb, each containing five grains, of which she took two immediately. They operated severely all the evening and all night; the discharges were dark. She was not pleased at being disturbed in the night, but admitted she felt better, and that the discharge was diminished. She was encouraged to try another dose; and then another; and before she had taken the whole, the discharge had entirely ceased. Moreover, all the symptoms which preceded it disappeared, and she became remarkably healthy, and continues so at this time.

The next patient in this disease treated in this manner, was a woman a little above fifty years of age, who had had this discharge to great excess for many years; she said for seven, her daughter said for a longer time.

Her menses were about departing when she had a very violent fever, and they returned in great quantity in the course of a long illness, and had continued ever since; not having

been entirely absent for one day, as she repeatedly declared in presence of several of the family, who confirmed the statement.

She complained also of ringing noises and continual pain in her head; of soreness in the pit of the stomach, and middle and lower part of the belly: her pulse was weak and intermitting occasionally; she had frequent cramps in different parts; her stomach craved chalk; she had no appetite, and was somewhat costive. The passages were dark even when she took salts.

This woman took twenty grains of jalap and ten of calomel, which operated the whole day. The same quantity was given seven times in eight days. The whole operated very freely. She told me afterwards, what I did not at the time understand, that the operation of no dose was over when the subsequent one was given; so that there was a continued operation for more than a week. The discharges were green, dark, or black.

The menstrual discharge, which had sometimes been as great as in the case of a lying-in woman, became rapidly less, and in a few days there were only a few drops occasionally. She was directed to take after this, night and morning, two of the following pills: twenty grains of aloes, and five of ipecacuana, made into six pills. They operated very freely. On one day there was a little bloody serum discharged, which ceased when the medicine began to operate; and in less than three weeks she was perfectly free from any sign of discharge.

About a month after the cessation, in very cold weather, she had a smart discharge for part of a day. She continued to take cathartic pills, sometimes of one kind, and at others of another, with a constant improvement of her appetite and health in every respect, and an increase of flesh, until she became quite a hearty woman.

Two years afterwards, on inquiring I found that she had had a few periodical returns of the menstrual discharge, which were pretty free; but for a long time there had been none whatever: she had also had something of the leucorrhœa.

The third person who was thus treated, was a single lady, of twenty or twenty-five years of age.

She was thin and pale, her complexion sallow; on inquiring into her general health, I found she had been troubled with headach, palpitation, weak back and bad appetite. In the preceding fall she had had excessive menstruation, and shortly after a very severe fever. The menses were not very regular, but very free when they occurred.

July 1st. I gave her twenty-four pills of scammony, aloes, and rhubarb.

July 20th. She had been greatly benefitted in every respect. The symptom which in her view constituted the disease, and on which her attention was fixed, viz. the menorrhagia, had disappeared in a short time, and she began to ride about. On riding she perceived a little discharge, and did so on this day, having rode eight miles on horseback. She had not taken the pills since the discharge disappeared. The other symptoms were all much better, and her general appearance was evidently much improved.

July 26th. She called to see me, and was not quite so well. She had been in town since the last date, visiting, keeping late hours, &c., which is very injurious to invalids. She had not, however, any return of the discharge, except a very slight appearance, which she would not have mentioned had I not made particular inquiry. She asked for a few more pills.

The next case was that of a woman thirty-six years of age, and the mother of a number of children. She was accustomed to labour about her house, was thin and sallow, had an indifferent appetite, had complained for a considerable time past, of headach, of a numb feeling in her head, and a buzzing noise in it, of giddiness, of soreness on pressing the epigastrium, of pain along the ribs, and in the back and hips, and of cramp in the fingers and toes. At the time of her first application she had no menorrhagia, but complained of the abovementioned symptoms alone.

She received twenty-four pills of scammony, aloes, rhubarb, and calomel; and by taking a few every day, so as to operate

about three times a day, she was entirely relieved of the four last named symptoms, and all the rest were mitigated.

Four months afterwards, in July 1823, when a greater epidemic than had occurred in this country for nineteen years, was fast approaching; and when patients in various chronic affections were the first to suffer, she applied again. In consequence of having ceased taking medicine when several of the most important symptoms had been only mitigated, she had relapsed into the state above described, and in addition to the symptoms there mentioned, menorrhagia had appeared. She had been suffering it for near three months, and it had become exceedingly profuse.

July 11th. I gave her twenty-four pills, made of two scruples of each of the following medicines, viz. rhubarb, aloes, calomel, and ipecacuana; of which she was to take two for a dose.

July 17th. The pills operated four or five times a day, for the first two days; then gradually less; and the last two days they operated but once. The first discharges were dark; the last was yellow.

The headach was much better; the thick and numb feeling of the head, the buzzing, and the giddiness, had disappeared; appetite indifferent, but craved meat; no complaint of pain in the back. By the second day the discharge of blood was quite weakened. On the 14th and 15th it became less and less; on the 16th she was entirely free from it.

Some little return to-day, the 17th; note, *there was but one passage in the last two days.*

Gave her twenty- four pills made of half a drachm of each of the following medicines, viz. jalap, aloes, rhubarb, and calomel.

July 25th. Not quite so well; had had a return of the menorrhagia; but is notwithstanding much mended in general health. I told her husband that I expected she had been worried over the fire on account of harvesting, and been up late. He said it was just so; that she had moreover rode to town twice; and when she was taken she had been all day at the wash-tub; *and the medicine had not operated.*

Gave her twenty-four pills of rhubarb and aloes.

July 29th. Her husband came to tell me that she was worse, the menorrhagia was very bad indeed; and that she looked very badly, and "was out of all heart."

She had still been over the fire preparing food for the harvesting people. *The medicine had not operated well, only about once a day*; the passages brown.

I made him recollect that the first medicine which had operated well, had entirely stopped the discharge; and that since they had ceased operating, she had become worse. I told him I had tried long enough the common mode of treating this disease, and knew it often failed; that this never yet had, and that I should persevere in it; that he might, if he chose, try others, but that I did not believe that the common mode of treating the case by opiates, would succeed with her. He now told me for the first time, that after his first application to me, when the menorrhagia came on, he had applied to another physician, who had been endeavouring for a considerable time in vain to remove the disease; but that she had become worse under the treatment employed, and that he had for that reason returned. On inquiry, I found from the size, colour, and taste of the pill, and from the constipation produced, that she had been taking opiates, among other remedies. I then gave him twenty-four pills of jalap, aloes, rhubarb and calomel; of which she was to take four every two hours until they operated well.

August 2d. The medicine had operated better than the other, but not well. She was better, but still occasionally discharged a good deal of blood.

Again I pointed out the facts, that when the medicine operated well, she was well; when it operated almost none, she was much worse; and now when it had operated better than it had latterly done, she was better. He assented to the whole as perfectly true, and took sixteen pills of scammony, aloes, and rhubarb, with directions to give four every two hours until they should act *well* on the bowels.

Aug. 4th. He informed me that his wife was much better, and that the medicine had operated *very* well. He said "she

took four of the pills that night, (August 2d, having taken other pills the same day,) and in two or three hours two more. They operated *very* well. Next morning she was better. Took four more. They operated well. At night she was a great deal better. This morning she took four more, and they operated well, and she has not had a sign all day." Gave him twenty-four pills of the same kind.

Aug. 10th. She is much better; scarce the lest appearance since the last application (August 4th), though she had been at her housework again. Gave him twenty-four more pills of the same kind.

Aug. 22d. Called again, and stated that his "wife was getting fat and hearty; so much so that her troubles are of another kind, lest she should be going to multiply: she has not been as well these ten years. Got very wet some days ago, and is costive."

Gave him thirty-two more pills of the same kind, with directions that she should take a dose occasionally.

The next case was that of a married woman of twenty or twenty-five years of age. She had a variety of complaints, such as have been stated above, and latterly excessive menorrhagia.

May 30th. Sent her four cathartic pills by her husband, who had called to see me and state the case. She sent word in an hour or two, that I certainly did not understand her husband, or he had not given me a correct account of her case; or I would not have sent her such medicine, and that she could not take it, as it would certainly kill her. I went to see her, and persuaded her to take the pills.

May 31st. The medicine had operated well, and she was so much better as to be willing to continue the medicine. Gave her twelve more pills, which put an end to the discharge.

A lady of a remarkably thin and delicate form, dark complexion, hair, and eyes, who had been long in ill health, complaining of a variety of the symptoms already mentioned, was at length seized with most violent menorrhagia. It had con-

tinued excessive for fifteen days when I was called, on the last two days particularly.

May 20th. She had been accustomed to have a sufficient operation from two pills. As the case was urgent, and I had always found that a free operation was most immediately beneficial, I gave her five pills of rhubarb, aloes, and calomel.

May 21st. They had operated pretty well, and she had less discharge. Gave her five more pills.

May 22d. The pills had operated again pretty well, and the discharge was still less, but yet considerable. Gave her four pills of jalap, aloes, rhubarb, and calomel, which, on account of the addition of jalap, were expected to operate better.

May 23d. They operated, however, only once well, and twice indifferently. The discharge stationary. Gave her six pills of the same kind.

May 24th. The operation still indifferent; only one good passage. The discharge was quite considerable. Her sister-in-law, a very intelligent lady, had visited her, and had told her that she had certainly not informed me of the true state of the case, or I would not use such medicines, but would give her astringents; others were very active in the business. The case had now continued twenty days, fifteen before I was called, and five since. I had but lately been consulted by this family, and had had no opportunity of doing much to acquire their confidence.

Under these circumstances I observed that astringents were the medicines which would be prescribed by every physician in town; that I had long used them myself, but that they were known often to fail; that I had given them up for my present practice for several years; that this course had been in every instance successful, and that I would answer for the termination being favourable if she thought proper to continue. She said she put herself in my hands. I brought to her recollection that the first two doses which had acted pretty well, had manifestly lessened the discharge, that the next two had operated only once well, and twice very indifferently, and that the discharge had during that time been stationary; and concluded

by saying, I therefore shall now give a dose which will operate *well*; and accordingly gave her four pills containing fifteen grains of calomel, ten of scammony, and five of rhubarb.

May 25th. The pills had operated all day very freely; the passages were dark. The discharge had almost ceased; but this morning there was some return. The operation, however, had commenced again, and continued all this day also to be pretty free. The discharge of blood was inconsiderable after the morning, and had nearly ceased.

The medicine continuing to operate so well, she took nothing this morning.

May 26th. The operation being over, the passages still green or dark, and there being still a slight discharge of blood, she took thirty grains of rhubarb, fifteen of calcined magnesia, and ten of calomel, at once, in a little sugar and water.

May 27th. The medicine had operated well: the discharges from the bowels being still dark, she took scammony and rhubarb, of each twelve grains, made into five pills.

May 28th. The medicine still operating, she took nothing.

May 29th. She took of scammony six grains, of calomel and rhubarb each four grains, made into three pills.

May 30th. The pills operated very well; the passages from the bowels were still somewhat dark.

She took three more pills of the same kind. The discharge of blood had entirely ceased for some days.

The last case I shall state was that of a woman about thirty years of age, short and of a strong make, who had had several children.

She had consulted me several times before, in the course of the last four years, having been in an indifferent state of health. She had complained latterly of palpitation, giddiness, and general weakness. She took some pills of rhubarb, aloes, and calomel, and was relieved; as soon however as she felt better, she ceased applying. She had now a profuse menorrhagia, which had continued for some days.

June 26th. Two pills ordinarily operated on her bowels: as the case was urgent, I gave her four. In six hours, there hav-

ing been no operation, and no diminution of the discharge, I gave her jalap and calomel, of each ten grains.

June 27th. The medicine had operated once copiously, and twice slightly. The discharge of blood had sensibly diminished afterwards, and she slept well; but about daylight it returned profusely, insomuch the she was thought to be dying. I found her at nine o'clock this morning discharging freely, the pulse weak, the skin, however, warm enough, but the breathing hurried. She took immediately twenty grains of jalap, and ten of calomel.

Afternoon. The medicine had operated five or six times largely; the passages were green; the pulse full; the discharge almost at an end. She took fifteen grains of jalap, and seven of calomel.

June 28th. The medicine operated well; the discharge of blood still very trifling. She took a cathartic of the same kind.

Afternoon. The medicine had not operated; the dose had been repeated at three o'clock. There was some increase of the discharge; but when the medicine began to operate, it ceased.

June 29th. Her mouth being somewhat affected, she took of jalap twenty-four grains, which operated but little.

June 30th. She took a dose of the same kind, which operated but little.

July 1st. No discharge. Took nothing.

July 2d. Some return of the discharge yesterday after the visit, and this morning. Her mouth being sore, took calcined magnesia alone, which operated very well.

July 3d. The discharge had entirely ceased.

In the following case, the hæmorrhage occurred three weeks after the birth of a child. This lady had been visited by a number of friends from a distance, and she imprudently exerted herself very much in making ready to entertain them. She was so unwell in consequence of the fatigue, that she requested me to visit her.

September 23d. I found her pale, sick, fainty, giddy, with

pain in the head, and considerable uterine hæmorrhage, of which she had had some appearance for the last eight days.

She had taken on the preceding day, two tea-spoonfuls of magnesia, which had operated three times. There had been no operation to day, and this evening the hæmorrhage had increased. She took immediately, four pills of scammony, rhubarb, and calomel.

Sept. 24th. Morning. The medicine had operated twice; the first time at two o'clock in the night: the first passage was black; the second of a lighter colour: the hæmorrhage had ceased. She looked much better; talked with life; and moved in bed with strength. She took four more pills.

Night. The medicine had operated twice; the passages were dark coloured. She felt still better.

Sept. 25th. Morning. She took four of the same pills.

Afternoon. There had been no operation; the medicine had made her very sick; about two or three o'clock there was considerable hæmorrhage, which alarmed her very much; from the alarm and the hæmorrhage she fainted, and was slightly convulsed, as often occurs in fainting; after this the hæmorrhage moderated. She took scammony and calomel, of each ten grains, in four pills.

Night. There had been several small black passages.

Sept. 26th. Morning. She had taken in the night several tea-spoonfuls of calcined magnesia. A moderate operation was kept up; the passages were black: she felt stronger; there was but little hæmorrhage. She took of scammony fifteen grains, of calomel ten.

Afternoon, two o'clock. Still a moderate operation; the passages black; she had vomited once; the hæmorrhage was trifling. She took fifteen grains of scammony.

Five o'clock. The operation more free; passages black; she had vomited considerably, throwing up green bile; the hæmorrhage had ceased.

Night. She had complained all day of a bad pain in her head, and back, and to night of a suppression of urine. She took fifteen grains of scammony.

Sept. 27th. Morning. The medicine had operated well; there had been three or four green passages; no hæmorrhage; had pain in the head, and sick stomach. She took ten grains of calomel.

She still complained of the suppression of urine: she had called it a strangury, which had occasioned its being overlooked, or rather left to be carried off by the medicine she was taking for the hæmorrhage. She now took some spirit of nitrous ether, thinking it would relieve her, and some other inconsiderable remedies, without benefit. She tried a cold application to the pudendum, with no better effect: at length, after having ceased from making efforts, she had a large discharge, as it were spontaneously.

Sept. 28th. Morning. A moderate operation still continued; passages green; less nausea; no hæmorrhage. She took fifteen grains of scammony, to which was added a small powder of jalap and calomel I had in my pocket, five grains of each.

Evening. The medicine operated well; passages green; she had vomited yellow bile; no hæmorrhage; she was considerably better.

She continued for a considerable time afterwards to take laxative pills; there was no return of hæmorrhage. During a considerable part of this illness, she could not rise even to use a bed-pan; but used cloths when the medicine operated. She, however, recovered a better state of health, with more flesh and colour than she had had for some years.

These cases are enough to show the success of the practice. There was no other medicine used in these cases than what has been stated. The practice has not failed in a single instance. On one only there rests a doubt of a perfect cure. The patient had been long suffering under the utmost degree of palpitation, &c. &c., and repeated and excessive floodings. She took four or five boxes of pills, and her daughter, who came for the medicine, finally told me she was almost well; and having given her another box, I saw no more of her. The result of the case I know nothing more of, as she had moved away, when some time after, in riding by, I stopped to inquire the termination of the case."

Excessive Menstruation.

A distinction has been made between menorrhagia and excessive menstruation. It is alleged that the menstrual fluid is not blood, but a secretion.

Secreted fluids are separated from the general mass of blood for the use of the body, or of its offspring, or for the relief of the system from a constituent part of the general mass, the retention of which would be injurious. Those which are for the use of the body, or its offspring, are provided when they are wanted; those which are always wanted are always going on; those that are wanted occasionally, are occasionally provided.

The menstrual blood is not secreted for any use: when it is *wanted* (and surely it is wanted when it is retained in pregnancy) it is not secreted; and therefore what is wanted is an unsecreted fluid, or the blood itself; and when it is not wanted it is thrown off: it is blood, therefore, which is thrown off, and not a fluid secreted for the purpose of the body or its offspring.

Neither is it a constituent part of the general mass separated because of injurious qualities, and not to be retained without danger. The secretion of those fluids which are thrown off on this account, is always going on; because, if they are injurious at all, they are always injurious. But this is retained generally a month, sometimes much longer, without any serious inconvenience.

It may be alleged that the retention does produce evil effects: true, but no other than the retention of any customary discharge of blood: the very symptoms produced by retention of the menses, are produced likewise by retention of the hæmorrhoidal blood, or of any habitual hæmorrhage: and the loss of blood from the hæmorrhoidal veins, or a discharge of black venous blood from the stomach, removes the symptoms produced by the retention of the menstrual blood.*

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* "Mulier sanguinem vomens, profusis menstruis, liberatur. Quæ menstruis non purgatur, si sanguinem ex naribus fudit, omni periculo vacat." (Celsi de Medicina, &c. Lib. II. cap. 8) A woman vomiting blood is freed from it by profuse menstruation. One who has no menstrual discharge is free from danger if a bleeding from the nose come on.

The menstrual blood, moreover, is designedly retained in pregnancy, which is full proof that it is not an injurious constituent part of the blood.

The menstrual fluid therefore is not separated for the use of the body or its offspring; neither does it consist of injurious constituents of the blood.

It is not, therefore, a secreted fluid.

It is however alleged, that it must either be blood, or a fluid separated from the blood, viz. a secretion; that it is not blood, because it does not resemble the blood drawn from an orifice in a vessel; and that therefore it must be a secreted fluid.

The difference between the menstrual fluid and blood is shown, it is said, by the difference of colour, by the menstrual fluid not separating into the different parts of which blood is composed, and by its not coagulating.

With regard to the colour, the menstrual blood is of a brighter red than venous blood, and not so bright as arterial; it differs therefore from both.

The blood in this case proceeds from the minute branches of the arteries, which in common do not carry red blood: just issuing from the arteries near the points where they are terminating in the veins, the blood is of course darker than in any other part of the arteries, and brighter than it is after it has continued longer in the veins. Blood from an orifice in a vein of the hand, is of a brighter red than that from an orifice at the elbow. Delay of the blood in the veins of the arm by a ligature but for a moment or two, produces a deepening of the colour of the blood visible through the skin. The menstrual blood is moreover darker when it is not discharged at the usual period: if longer retained it is often black.

It is evident, therefore, that the colour of the menstrual blood affords no ground for this doctrine; but rather shows the reverse, inasmuch as the difference observed is just what it ought to be, when we consider the vessels from which it flows.

The other two points of difference are but one, inasmuch as it is the act of coagulation that separates the parts of the blood.

In this respect, however, the menstrual blood differs not from

that often discharged from the bowels. I have seen it often, and have had it kept for hours, sometimes all night, until the forenoon visit, and have found it still fluid and black. The same want of power to coagulate is sometimes observed in the blood drawn from the arm. The menstrual blood therefore is not more a secretion on this account, than the blood generally discharged in hæmorrhages from the bowels, or the whole mass of blood in certain morbid states of the system.

The discharge often commences with blood which does not coagulate, or with common menstrual blood; and in the course of a few days or hours, sometimes with and sometimes without any observable cause, it is observed to coagulate. In this case menorrhagia is said, by those who favour the distinction, to have supervened during, or at the time of a menstrual period. But the fluid discharged, whether it coagulate or not, is unquestionably from the same source, passes from the same vessels, and is the same fluid with an unessential variation of colour, &c.

The distinction is of no practical importance. Excessive menstruation is treated in the same way precisely with menorrhagia, by those who make the distinction.* In the cases above cited no distinction was made, or indeed once thought of.

To conclude, this discharge is not separated from the mass of blood for the use of the body, nor for that of its offspring; it is not separated because of any noxious qualities requiring its separation; it differs in nothing material from the general mass of blood, in nothing from the blood discharged in other hæmorrhages, nor even from that sometimes drawn from the arm; sometimes in the same case the blood discharged is at first such as is distinguished as menstrual blood, and afterwards such as is considered not menstrual, by those who make a distinction between excessive menstruation and menorrhagia; the distinction leads to no difference of practice in those who make it, or those who do not; and therefore it does not exist; and even those who contend for its existence must allow that it is useless

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*System of Midwifery by W. P. Dewees, M. D. Compare paragraphs 364 and 368, with paragraphs 999 and 1021.

to make any other inquiry of the patient, than the simple one, Is there an excessive discharge of blood from the vagina?"

Of Menorrhagia during pregnancy.

When menorrhagia takes place during pregnancy the child is generally lost without proper assistance. The writer has been induced by reasons detailed in the work above referred to, to believe that the same proximate cause produces this hæmorrhage in persons not pregnant and in those who are. Resting confidently on this conclusion, and confirmed by the successful issue of the practice of evacuating from the liver in the latter, I gave the same cathartics in the first case of the former that occurred, and with the same complete success.

"The first case of this kind so treated, occurred in a lady of dark complexion, and black eyes; her complexion was sallow at the time; she was thin and delicate, frequently complaining of a fainty feeling about the præcordia, pain in the back, &c.

The discharge appeared in the end of the fourth month of her fourth pregnancy. I found her cool, with a moderate pulse, and not much alarmed. She immediately took four pills of scammony, rhubarb, and calomel; and in the course of the two following days, twelve more. They all operated well: from the first operation the discharge ceased, and returned no more. On the fourth day I gave her twelve pills, to take occasionally, and left her. Five months afterwards I attended her in labour, and she had a very fine child which is now living.

The second was a remarkable case in several respects. The woman was about thirty years of age, uncommonly gross. She was pregnant the seventh time. She had menstruated for the first four months in every pregnancy; she particularly said, on being closely questioned, that there were no clots of blood passed. In every instance she had been affected, about the beginning of the ninth month, with pain in the lower part of the abdomen, in the back, sometimes in the head, with loss of appetite, sick stomach, and costiveness: after three or four days or a week, in addition to these symptoms she had a discharge of blood from the vagina, which she called by the common name,

flooding. This continued with more or less violence for two, three, or four weeks, until she was delivered. The children were born alive in every instance but one; but had no appetite, or very little, soon fell away, had a discharge of black blood from the mouth, and died within the month. This was the case with six children.

In her seventh pregnancy she menstruated as long as usual in the commencement; and in the beginning of the ninth month, was attacked as usual. Being requested to visit her, I found her with pain in the abdomen, head, and back, and with a considerable hæmorrhage from the vagina. The cathartic pills were given her, and the effect of the operation in checking the discharge of blood was evident. She was, however, very ungovernable, and would not take the medicine regularly; nor would she take any thing but that which suited her own caprice. In this she was countenanced also by her sister. I should have left her, but anxious to see the effect, persevered. She finally lost the child in the same way with the others; but the medicine had so obvious an influence over the hæmorrhage, arresting it whenever there was a free operation, as to convince her husband, and make him very angry because she would not persevere, as he had no child, and wanted one.

Twenty-seven months afterwards, being in the commencement of the ninth month of her eighth pregnancy, after having menstruated as usual for the first four months, the usual symptoms came on; viz. pain in the abdomen, &c., loss of appetite, nausea, together with vomiting and purging. At this time, the weather being very hot, the cholera morbus was common among children and adults. She immediately applied for assistance, in hopes of preventing the usual hæmorrhage, of the probability of which I had given her and her husband the strongest assurances.

She took a cathartic immediately, which operated very well: on the second day she took another, which likewise operated very well: on the third day her bowels were so loose as to render it unnecessary to give her any. The loose state continued; and she had three or four good passages every day for the whole month: she had no hæmorrhage whatever, and at the full time had a fat, hearty child, which is still living and in good health.

The next case was somewhat similar to the foregoing. The woman was about thirty years of age. She had lost a number of children towards the end of her pregnancy, and had not one living. In her last pregnancy she applied to me to prevent this. She was then beginning to flood smartly. She took without delay, four cathartic pills; on the next day five; on the third day none; on the three following days sixteen. The discharge ceased almost entirely at once; but the passages being dark and green, the medicine was continued, and on the seventh day I left her with a dozen pills, to take a few occasionally.

Near three weeks after that time, I was called to see her again. She was sitting in bed, flooding freely, and had severe pains with intervals of ease. She considered herself in labour, and called on me on that account. Every appearance indicated that this was the case. I however determined to try the effect of a cathartic, and gave her immediately, four pills of jalap, aloes, and calomel; which not operating satisfactorily, she took twelve grains of calomel in a few hours after.

August 3d. The medicine had operated well, the passages were green, the pains had ceased, and the discharge of blood was inconsiderable. She took ten grains of jalap, and ten of calomel.

Aug. 4th. The medicine had operated well; the discharge of blood had ceased entirely: the passages were green; she took the same powder.

Aug. 5th. The medicine operated well; there was no discharge of blood; the passages were green; she took the same powder.

Aug. 6th. When I called, she had walked out. The same thing occurred the two following days. She continued well, and had some time after a child, which is now two years old.

A young lady, the daughter of the lady who made the observation respecting astringents, above stated, immediately after the termination of that case, having, by their reckoning, passed the time of delivery, was suddenly attacked by a considerable discharge of blood from the vagina, accompanied by pain. Being called in a hurry, I found the family under an impression

that labour had commenced. The hæmorrhage had ceased, but there was every reason to believe it would speedily return; and as there was by no means any certainty that the regular period of labour had arrived, I determined to regard the hæmorrhage alone. She therefore took immediately four pills of jalap, aloes, and calomel; on the second day four more of rhubarb, aloes, and calomel; and on the third, four of rhubarb and aloes. The hæmorrhage did not return; she was entirely relieved from all pain, &c.; and on inquiring into the circumstances of her pregnancy, I made the calculation that she should expect the labour between the middle and end of the ensuing month. Accordingly, twenty-five days from the time of the hæmorrhage, I was again called, and she had a son after a favourable time.

During the epidemic of 1826, a lady three months advanced in pregnancy, sent me word that she had a discharge of blood from the vagina. She was requested to take a double dose of the pills of rhubarb, aloes, and calomel, which she generally kept in the house. On visiting her next day, I found the medicine had operated well, and the hæmorrhage had totally ceased.

She was advised to take a similar dose for three or four days, which she did, and had no return of the hæmorrhage. Six months afterwards I attended her in labour, and she had a very fine child; and had unusually good health afterwards.

The lady whose case is stated p. 388, twenty-two months afterwards, in the ninth month of her third pregnancy, complained of a pain in the back, becoming more severe at intervals. She was advised to take a dose of the pills of rhubarb, &c. which she had in the house. She put it off till bed time. About dark she had a sudden discharge of blood from the vagina; the pain had increased; and there were evidently such pains as occur in labour. She considered it premature; and encouraged by the result of former cases, I gave her immediately half a drachm of rhubarb, and a scruple of calomel. She always required a large dose. For a full hour after, there was no hæmorrhage with the pains. The simple fact is mentioned to show that there is nothing in the dose to increase such a hæmorrhage. After an hour had elapsed, the hæmorrhage ap-

peared again during the pains. These increased fast, the os uteri was well dilated, and quite soft and relaxed; and as she was much alarmed, having lost a sister-in-law a short time before whose labour commenced with flooding, the ergot was used, and the labour was over in fifteen minutes. The medicine did not operate at all. She took, every day for a week, the pills of rhubarb, &c., and recovered without fever, or any disagreeable occurrence.

The following case does not strictly come under this head, of menorrhagia during pregnancy; but is nevertheless so intimately connected with the subject, that I shall state it.

A young lady, about the middle of the ninth month of her first or second pregnancy, had severe pain in the back and abdomen, returning at pretty regular intervals. She called a midwife, who considered her in labour. She continued three days in this situation; and at length desired me to visit her. Though the straining had been violent, there was no hæmorrhage. The head of the child was forced pretty low down, but the os uteri was still closed.

She took half a drachm of calomel at once; and as soon as a free operation was affected, which was in a short time, she became quite easy. The discharges were green. She continued to take mercurial cathartic pills, &c., for five or six days, until the passages became quite natural. The doses necessary to operate were in this case very large. No hæmorrhage occurred; the straining did not return; and on the fifth day from its disappearance, she had a child after an unusually short labour.

These are all the cases that have occurred since this mode of treatment was adopted. They show in the most decided manner, the good effect of cathartics.

In every case in which hæmorrhage occurred without uterine efforts, it was carried off by the use of cathartics, and the child was saved.

In the only case in which the hæmorrhage was not carried off entirely, the patient did not use the cathartics as directed: nevertheless the good effect of those she did use was so marked, that her husband was convinced that she would have been re-

lieved entirely, if she had persevered; and she herself showed afterwards that, she was convinced she had been following bad advice, by applying on the very first symptom of a similar attack in the next following pregnancy.

In the last pregnancy the hæmorrhage was evidently prevented by the use of cathartics and the child saved, although she had lost every one of seven preceding children with the same symptoms precisely, as far as they were allowed to proceed.

In one case, in which free hæmorrhage was accompanied by such efforts that the woman was considered in premature labour, both were arrested by the use of cathartics, and the child saved, although she had lost every preceding child precisely in this way.

In one case, in which, in the opinion of an experienced midwife and several ladies, premature labour had continued for three days, without hæmorrhage however, and without dilatation of the os uteri, the painful efforts were carried off entirely by the use of cathartics, and the child was not born until the full term had expired.

These cases show in the most decided manner the perfect safety of giving cathartic medicines to pregnant women. The writer has derived great advantage from using them in many other affections of women in that state. There is indeed scarcely one of the numerous symptoms they complain of, that is not relieved by two copious evacuations from the bowels every day. The disordered state of the stomach and bowels, the stitches and pains in the back and sides, the vertigo and headach, the heavy, unwieldy, listless sensations which so often distress them, the hæmorrhoidal affections, even the low spirits, are prodigiously relieved by such treatment.

A lady, about 30 years of age, who had several children, had been for two years in a bad state of health, and had fairly tried every thing that could be devised by an eminent practitioner, of great age and experience, who treated her on the old plan of administering tonics, without any benefit. He at length even told her husband that he could only "patch her up" for a while and that she could not live long.

She was then in the fifth month of pregnancy. She was very much emaciated, very sallow, weak, and excessively low spirited; and frequently had a violent trembling like an ague. In this situation she lay the greater part of the day wrapped up in blankets. On being requested to visit her I advised her to adopt the plan proposed above. She began immediately to take pills of jalap, aloes, and calomel. It required five or six to operate. The passages were dark. Satisfied even by this, that the plan must be attended with advantage to her, I urged her to continue the medicine regularly. She did so most faithfully. In a few weeks her appearance was so much improved as forcibly to strike the attention of all her friends. She was so much improved in spirits as to run up and down stairs, singing in the most cheerful manner: she gained flesh and colour every day; and at the full period had a fine child after a remarkably short and easy labour. It is to be remarked that the medicine uniformly vomited her severely, for a week or two after she began to take it; a circumstance I was not informed of for some time afterwards.

A lady of very full habit, who lived well, in the eighth month of pregnancy desired my assistance. She had a constant leucorrhœa, and occasionally a discharge of blood instead of it; she was much troubled with the hæmorrhoids; her stomach was very much disordered with acidity and flatulence; her head, with vertigo and pain, and sometimes considerable stupor; she also had frequent erysipelatous flushings and eruptions. These symptoms had in a preceding pregnancy, increased continually until at length she was prematurely delivered of a dead child.

She was advised to use the pills of aloes, rhubarb, and calomel every night. She did so, and the symptoms were immediately mitigated, and in a few weeks carried off. At the proper time she had a child, which lived.

A lady about the age of 30, small and delicate, who had had several children, and who had for some time been in a very bad state of health, desired my advice. She was very feeble, of a pale, or what is called leucophlegmatic complexion, with œdema-

tous swellings of the feet and legs, and evident tendency to general dropsy, with frequent palpitations, disordered stomach, and leucorrhœa.

She commenced taking pills of aloes, rhubarb, and calomel, (two pills containing four grains of each of the former and two of the latter) every night. She soon improved, and laid them aside; and relapsed. She then travelled extensively for some months. Returning home without the least improvement, and now far advanced in pregnancy, she commenced taking the same medicine regularly. She improved in a short time visibly, and persevering in some weeks became quite hearty, and recovered a very fine state of health.

It has been repeatedly mentioned that those who took medicines of this description had easy labours. This has been a common observation, not of one person only but of numbers. There is nothing more certain than this effect of these medicines, of which it would be easy to cite many instances. It is however time to close this essay; the object being to turn the attention of practitioners to the facts stated. If this should be attained every one will soon be convinced from his own experience, not only of the safety, but of the immense advantages resulting from the practice recommended.

ART. V.—*An Essay on Autumnal Remittent Fever.* By Elijah Coons, M. D. of Tuscumbia, Alabama.

FEVER is the most universal, and most fatal disease of which mankind are susceptible. It is therefore the most important. And though more has been written on it than on any other morbid affection, yet there is no one subject in the whole circle of medical science which involves so many disputed points. From the earliest dawn of the science down to the present day the medical world have been divided in opinion on this momentous topic. Nor is this remarkable when we recollect that autum-

nal remittents vary with the nature of climate, season, constitution of the patient, and local circumstances.

The nature of fever, and the modus operandi of those agents that excite it, still afford grounds for speculation; and the treatment of it is far from being settled. But the great improvements that have been made on this subject, within the present century, mark an important epoch in the history of medicine.

In the following essay I propose to give some account of autumnal fever, as it appeared in Lawrence county, Alabama, in 1827 and '28.

In a previous paper* I have attempted to show the remote cause of this disease to be a gaseous poison, "the product of vegeto-animal decomposition;" for if there be any theory in the science of physic based on undeniable facts, and substantiated by irrefragible evidence, it is that of the existence and deleterious effects of malaria. Hippocrates was of this opinion. Lind, Dalzelles, Cleghorn, Pringle, Mosely, Trotter, Johnson, Macculloch, and a host of others concur in this belief, and not without the most conclusive testimony. Nor is this opinion confined to medical men; "the inhabitants of the fens of Lincolnshire, the Maremma of Italy, or the morasses of Ceylon," attribute (and very justly) the great waste of life among them to this widely pervading and pernicious gas.

But why travel over to Europe to search for miasmatic districts? The citizens of the towns of New-Orleans, Natchez, Norfolk, Charleston, and Mobile; and those of the states of the Carolinas, Georgia, Alabama, Mississippi and Louisiana, will point you to low, damp, and marshy places, as the chosen thrones of malaria. Those situations, in the summer and fall, invariably teem with morbific effluvia, and are as uniformly (more or less) sickly. This fact is so notorious that those places, though occupied in the first settling of this country, are now generally deserted; and new comers are admonished to avoid them, as the prolific sources of disease. We do not pretend to say these situations are the only ones that give origin to fever, but here it most fre-

*See Transylvania Journal of Medicine, Vol. 2. No. 1.

quently occurs; and the places most exempt from the causes of decomposition are likewise the most healthy. But to return from this digression.

By tracing the phenomena of fever, we are persuaded that it is caused by marsh effluvia, taken into the stomach, and primarily operating on this organ as an irritant. The "primary local impression" being thus made on the mucous lining of the stomach and intestinal canal is, from the close connexion of this viscus with the balance of the animal economy, sympathetically extended to the general system, which is thrown into agitation and perturbation, and fever is the consequence.

It has been contended, however, that the lungs constitute the avenue through which these pestiferous effluvia pass into the system, or are the point on which they make the primary morbid impression. Professor Chapman* thinks this very improbable. And if we mistake not Professor Caldwell dwelt on this subject at some length, in his very excellent private lectures on the theory and practice of medicine, in the winter of 1824–5, to prove the improbability of it. But the limited scope of this essay will not allow a free discussion of this topic here; I shall therefore pass on to the description of autumnal fever as it occurred to my observation in 1827 and '28. And for the sake of conciseness, method, and perspicuity, I shall follow the arrangement of Dr. Armstrong; viz. into the simple, inflammatory and congestive.

These distinctions are of great practical importance, and are now sanctioned by all enlightened practitionres and physiologists.

The simple or excited variety being the most common and least dangereus will be noticed first. It is marked in its forming stage, or stage of oppression, by the annexed symptoms: the skin is pale and shrunk, and the extremities are cold; whitish tongue on the middle, its edges clean and of a scarlet red; small, quick, weak, and variable pulse; hebetude, lassitude, anorexia, vertigo, confusion of the sensorium, fugitive pains through the

*See his Therapeutics.

body, constipation, with frequent alternations of hot and cold fits. In a day or two these symptoms were generally succeeded by the development of the stage of excitement; sometimes, however, the fever was ushered in by a chill without any premonitory symptoms.

Now the pulse becomes frequent, full, and resisting; the cheeks are flushed; mouth dry; insatiable thirst; skin hot and dry; tongue foul; and great anxiety of countenance. The duration of this stage is governed by the mildness or severity of the incursion of the disease. There is, however, a remission generally in the morning, and an exacerbation returns in the evening. Such are the leading features of that form of autumnal fever now under consideration. When the appropriate measures are timely adopted it readily succumbs. But if medical aid is not solicited for several days (which is too often the case in this country) till some particular organ has become affected, the case proves more lingering and troublesome.

We may augur well of the case if the symptoms gradually subside; and the skin becomes soft and comfortable with a softness, freedom, and easy expansion of the pulse. These phenomena are manifestations of centrifugal action and a restorative normal function.

Most cases that terminate favourably are marked by a bilious or sudatory discharge, which by some authors has been denominated "*a critical discharge.*" But some cases convalesced without any evident crisis.

When the disease marches onward in its course in defiance of medicine, the symptoms are restlessness and jactation; a weak, quivering, and undulating pulse; respiration feeble and anxious; tongue dark and parched; hippocratic countenance; subsultus tendinum; low muttering delirium; a cadaverous smell; and coma. These are the immediate precursors of death.

To preserve order, we shall next endeavour to portray the inflammatory variety; to enumerate the symptoms of its first stage would be a mere rehearsal of those in the preceding or simple form. But when the stage of reaction is confirmed, the febrile phenomena are more violent and aggravated; and the

pulse imparts a different sensation to the finger; in some cases it is full, quick, and tense; in others, small, frequent, and corded.

The parts that suffer most frequently from inflammation are the brain, lungs, liver, and spinal cord. The cerebral department being the most important, both in texture and function, demands the first notice. Inflammation of the brain is designated by intense pain in the head; strong determination of blood to the encephalon; suffusion of the eyes; strabismus; intolerance of light; with a quick, hard, and irregular pulse, and a great aversion to shaking the head. These are among the first symptoms. As the disease progresses, watchfulness; disordered intellect, evinced by a stupid fatuous stare; intoxicated cast of countenance; delirium, and coma ensue; and if it is not arrested the tongue becomes of a dark brown; the eyes nearly insensible, with dilated pupils; tinnitus aurium; tremors of the hands; dull moaning; hæmorrhage from the nose, ears, &c. with involuntary stools and a cadaverous sweat, close the scene in death.

Pulmonic inflammation generally comes on with pain in the chest, increased by a deep inspiration; sensation of stricture across the breast, with laborious and painful respiration; countenance of intense anxiety; dark and livid glow of the cheeks; tongue coated with a brown fur; and the pulse frequent and irregular.

When the liver is the seat of inflammation, the patient complains of fulness and pain in the right hypochondrium, pain in the shoulder; nausea and vomiting; tongue covered with a yellow fur; pulse quick, restricted and elastic; torpidity of the bowels; the alvine discharges pale and argillaceous; and sometimes the skin and urine chlorotic from a regurgitation of bile, morbidly secreted, into the vascular system.

Inflammation sometimes invades the stomach and bowels, and constitutes one of the most formidable occurrences of this variety of fever. It is marked by nausea or vomiting; ungovernable thirst; great heat and soreness of the epigastric region; pulse feeble, irregular, and inelastic; peculiarly haggard and anxious countenance, &c.

If the spinal cord be implicated, it is indicated by pain along

the spinal column; soreness over every part of the body; peculiar tingling sensation in the extremities, sometimes with partial paralysis; dysuria; pulse small, quick, and tense, &c.

Having attempted to delineate the complex or inflammatory form of fever, and having enumerated the usual symptoms occurring when the most important organs are affected, we shall dismiss this part of the subject at present; conceding, "en passant," that other parts are frequently involved, and require the appropriate treatment, which every educated practitioner will readily understand.

The congestive form.—This modification of autumnal fever differs from the two preceding in the engorgement of the viscera in the stage of oppression, which continues, if not removed by the application of the expedients of art, and prevents reaction; and the stage of collapse comes on without the intervention of the stage of *excitement.* The most violent and dangerous attacks of this form of fever are marked by preternaturally cold skin; cutis anserina; universal lassitude and weakness; the head confused and vertiginous; vision glimmering and indistinct; pulse unresisting, struggling, and variable; and the powers of locomotion partially paralyzed. In this abject state of life, it is the paramount duty of the practitioner to excite reaction, if possible; to effect which he must be governed by circumstances, and select those means that appear the most effectually calculated for this purpose. If reaction cannot be produced the patient sinks into a lethargic state from which medicine cannot recall him. But when the congestion is less extensive, the case is more tractable and the patient may be relieved. The organs most commonly affected in this variety are, the brain, the lungs, and the liver; if in the first, there will be cerebral oppression, stupor, and delirium; if in the second, a laborious weight on the chest, with restricted and anxious respiration; if in the third, the bowels are torpid; alvine evacuations of an ash or clay colour; pulse, particularly in the left arm, oppressed and confined, and the skin cold and chlorotic in appearance. The duration of this form of fever is variable, depending for the most part on the degree and extent of the existing congestion.

Treatment.—Having portrayed, under its different forms, the fever now under consideration, I shall immediately proceed to give the mode of treatment to be pursued to subdue it. In the mild cases evacuations of the primæ viæ and a restriction to the antiphlogistic regimen are frequently sufficient to produce a solution of the disease. But if the incursion be violent and any important viscus becomes implicated, the treatment must be modified accordingly.

Blood-letting.—Though the ancients understood but indistinctly, prior to the discovery of Harvey, what agency the circulating system, and its contained fluid, had in the production of the phenomena of disease, yet they were in the habit of resorting to the lancet as a remediate agent.* Since this æra in the science of medicine, it has been employed upon physiological and pathological principles. And now, in febrile disorders, the abstraction of blood is considered one of the most potent means for fulfilling the indications of cure. In a large majority of the cases of the first, or simple variety of autumnal fever, the lancet may be dispensed with; but if the onset be violent, and the patient is plethoric and of a vigorous habit, and the pulse is frequent, elastic and full, or tense and quick, blood-letting may be resorted to with signal efficacy. The copious abstraction of blood at this period will frequently cut short the disease, or, at least prevent local congestion, and consecutive organic læsion.

If some local affection exists at the time of the attack—and particularly when it has been the cause of it—liberal depletion of the sanguiferous system and alimentary canal is indispensably requisite, because here we not only have to contend with the fever, but to ward off inflammation from the infarcted organ.

In the progress of fever, congestion, as already observed, may take place in the larger and more important organs of the animal frame, as the brain, lungs, liver, &c. Dr. Fordyce tells us it is a truism that "nothing is more dangerous in fever than its

*Venesection was performed on the Carian princess Syria 1180 years before the Christian æra, by Podalirius.

affecting one part more than another, and *especially* if the organ affected be large and important." Such cases must be combatted by the free use of the lancet, which must not be relinquished until it has attained its object.

In the most violent congestive cases, as above stated, it is the paramount duty of the practitioner to excite reaction, if possible; to effect which, evacuations both from the blood vessels and primæ viæ are requisite. To drain off that superabundant fluid from the oppressed organs of life, the lancet should be cautiously used; if the powers of life increase and the pulse rises and expands, blood may be drawn freely; if the reverse happen, it must be suppressed, and gentle stimuli administered to obviate further debility. By thus unloading the oppressed circulating apparatus of part of its nearly stagnant fluid, it is enabled to contract and propel the blood onward to the exterior. The lungs, being relieved of embarrassment, ærate or vitalize the blood, which is thus rendered the heathful stimulus of the heart, and it being relieved of its burden, is empowered to perform its normal function; a general and equable distribution of the vital fluid ensues, and hence reaction is produced.

But it has been gravely asked "who but the enthusiast would think of resorting to the lancet under such circumstances?"* What magical change can take place in the system, in so short a time, as to render a single phlebotomizing "productive of the most fatal consequences?" The ghostly phantom, debility, which was so much dreaded by the Brunonians, still fetters the minds and paralyzes the hands of some individuals in the treatment of this variety of fever. But the collateral lights of physiology, pathology, and therapeutics must ere long dispel the mist of prejudice to the use of the lancet in such cases.

In the inflammatory form the advantages of venesection are now sanctioned by all educated practitioners. Its employment is indispensible. It must be repeated in such cases as long as arterial activity requires it. If there be a phlegmasial pro-

*See Heustis on Epidemics of Alabama in the 3d. No. of the American Journal of the Medical Sciences, for May 1828.

clivity in the system, it must be warded off by the use of the lancet.

The secretions are generally perverted and restricted in fever. Blood-letting opens the emunctories and capillaries of the system, removes plethora of the larger vessels, and favours the operation of medicine. In cases where the bowels are obstinately costive, and large and frequent doses of purgative medicine have been given without avail, the abstraction of blood ad deliquium will generally cause it to operate kindly and copiously. Here the lancet may be considered the key to the bowels.

I shall next advert to the use of the lancet in autumnal remittent fever, to prevent its degenerating into the typhoid type. It sometimes has an inherent disposition to run into this stage. This is one of the characteristics of the fever now under consideration, and particularly in 1827. Indeed medical aid was not solicited, in many cases, till it had run irretrievably into this form.

The strong advocates of the lancet declare it to be the sheet anchor of hope and only haven of safety in such cases; whilst other physicians of much respectability condemn its use, and rely sanguinely on other remedies. That the lancet opportunely resorted to, may prevent the typhoid state in some cases, no one of experience will deny. But here, as in many other cases, a medium course we believe to be the most correct. In avoiding Scylla, an eye should be kept to Charybdis; and, though blood-letting renders the case less formidable when typhus symptoms ensue, yet it will not always prevent them. I have seen cases so strongly predisposed to degenerate into this type, that neither mode of treatment could prevent them.

Before leaving this part of the subject, we propose making a few remarks respecting the correct mode of drawing blood.

1. It is a desideratum to induce faintness or syncope by the loss of as little blood as practicable; the object of letting blood being to make a permanent impression on the general system, and the capillary system in particular, at as little expense to the powers of life as possible.

2. To accomplish this it must be taken from a large orifice, in a full stream, the patient being in an erect or inclined posture.

3. It must be repeated so as to reduce abnormal action of the arterial system. To fulfil this object it is, sometimes, necessary more than once in the same day.

4. It is also sometimes necessary when the pulse is not above the natural standard; viz. when there is inflammation of the stomach and bowels, brain, liver, or lungs.

5. It must be confined principally to the early stages of fever.

With these aphorisms, I shall close my remarks on this part of the subject, hoping they may be found of practical utility, at least to the younger brethren of the profession.

Emetics.—This class of medicines has been employed as therapeutic agents from the earliest times. "To evacuate the stomach for remedial purposes seems, indeed, to be an instinctive impulse. It is a practice pursued by the most savage tribes, and even the brute creation." Nevertheless, there is some diversity of opinion in regard to the utility of emetics in autumnal fever.

When administered with the requisite precautions, however, I have invariably found them not only safe, but exceedingly salutary. In the forming stage they frequently cut short the disease at the very threshold. They do not only unload the stomach of its acrid or vitiated contents, but frequently discharge a greater quantum of bilious matter than could be carried off by cathartics in several days. They likewise determine the blood to the surface and in this way relieve the oppression of internal organs; and favor the operation of purgative medicine, which, if exhibited after full vomiting soon exonerates the intestinal tube of its scybala. Thus are opened the sluices of the excretory system, and that fœcal matter, which had been pent up and caused so much distress and suffering, is carried off. And thus the sensibility and contractility of the stomach and collatitious viscera are aroused, and the concatenation of the disease is broken up; and consequently harmony and the healthful function of the system are restored. The construction of the human frame is such that no one part can be materially

affected without reacting on other parts. Thus through a dependence of function, or sympathy if you please, the general system becomes diseased, and vice versa.

We have said emetics are to be given with certain precautions; and must confess when thus administered we consider them invaluable remediate agents.

1. They are to be given to clear the stomach of its noxious contents, and prepare the way for the reception of other remedies.

2. They are to be exhibited during the remission of fever, or as it is going off.

3. If there be much force of vascular action, or a phlegmasial diathesis in the system, the use of emetics should be preceded by venesection; and they should be made to nauseate a great deal before they vomit.

4. They may be given twice or thrice in twenty-four hours, in some cases, with unquestionable utility.

5. It is a mistaken notion that they should be confined to the first stages: they may be given with unequivocal benefit at almost any stage of fever.

6. When there is nausea, weight about the præcordia, giddiness, coldness, succeeded by slight fevers, with a low pulse, emetics are highly beneficial; and, if immediately followed by an *active* cathartic, they will sometimes cut short the disease.

7. If the thoracic viscera sustain the onus of the malady, after letting blood, they are peculiarly serviceable. They dislodge phlegm from the bronchiæ, and exert strong influence over the vascular system, and particularly over the capillaries of the affected organ and those of the periphery.

8. In protracted cases of autumnal fever, diarrhœa, sometimes, supervenes and is accompanied with vitiated secretion of the stomach and bowels. In such cases, gentle emetics, administered so as to operate speedily without prostrating the patient, are of great benefit. They should be succeeded by aperients, the warm bath, flannel rollers round the body, and lastly opium in conjunction with ipecac, and creta preparata to

correct the concomitant derangement of the stomach and bowels, to complete the cure.

9. They are contra-indicated in malignant cases and when there is much gastric irritability, or epigastric tenderness.

Cathartics.—There is no class of of medicines in the whole catalogue of the materia medica so universally employed in the treatment of disease as cathartics, and they have been used in every age and country; and are *now* mainly depended upon in the treatment of autumnal fever. "The intimate connexion existing between the alimentary canal, and other portions of the complicated structure of the animal machine, gives to them an extensive influence, and renders them among the most important means in the practice of the profession." They answer a three-fold purpose; viz. to cleanse the alimentary canal of offensive matter, unload congested organs, and reduce vascular action.

In the first variety of fevers purgatives are almost exclusively relied upon; and in the second, or inflammatory form, they are equally beneficial and may be used with venesection. Here free purgation simultaneously is indispensably requisite.

Irritability of the stomach sometimes occurs in the course of fever, and may justly be considered one of the greatest difficulties that we have to encounter in the treatment of the disease. Here our chief reliance is placed in a combination of opium and the protochloride of mercury. It is absolutely necessary to allay the irritation of the stomach, and nothing answers a more valuable purpose than the opium thus administered. The opium composes the stomach, and the calomel emulges the biliary system, which, in most cases, is an indispensable requisite. Soda water and a few drops of vinum opii may sometimes be beneficially employed. And if there be much epigastric soreness, and the irritable condition of the stomach continue, an epispastic applied over the scrobiculus cordis is advisable.

We are fully aware of the fact, that emetics have been advised, with certain precautions, in these cases, by a highly distin-

guished public teacher and useful practitioner of medicine; but with due deference to his opinion we concieve they would exasperate the stomach, and add greatly to the distress and danger of the patient.

In congestive cases, with the use of the lancet, warm bath, friction, and rubefacients, purgatives that procure large consistent bilious evacuations are imperiously called for. Exceedingly large doses of purgative medicine are sometimes necessary, and should be repeated until they produce the effect desired. When thus administered they will procure copious discharges of black offensive matter, which invariably give relief to the patient. There is no danger of prostrating the patient by free purgation, but on the contrary his strength and powers of life are manifestly augmented by it.

Hæmorrhages of the stomach and bowels sometimes occur in these cases, but this is rarely the case, if the proper evacuations have been premised.

In the winter of 1827 I was called to see a negro boy, aged about 14, belonging to Col. J., that had been attacked with the (then) prevailing endemic four or five days before. The symptoms from the incursion, as I was informed, were, stupor; coldness of the surface; a weak, inelastic, and variable pulse; tongue coated on the middle with a whitish fur; countenance vacant and contorted; incoherent expressions; great torpitude of the bowels; tinnitus aurium, &c. There was no particular alteration in the symptoms when I saw him. He had taken the most drastic purgatives without the desired effect. Great doses of cathartic medicine were continued, to move his bowels; and the hot bath, frictions, rubefacients, blisters, and diffusible stimuli were all regularly employed but to no effect. The blisters made no more impression on the extremities than if they had been bone. The second day after I saw him, pulmonic, nasal, aural, cystic, and intestinal hæmorrhage came on, and these with other symptoms, such as low muttering, twitching of the tendons, cold sweat, and coma denoted a fatal catastrophe was near at hand, and in a few hours the tragic scene was closed in death. Would

not venesection and catharsis at the onset have overcome the congestion and relieved the patient?

When remittent fever degenerates into the the asthenic or typhus stage, drastic purgatives would be highly prejudicial: But in this stage all offensive and bilious accumulations must be daily removed with mild cathartics and enemata. At night a preparation of Dover's powder, calomel and camphor, I have found to be an excellent combination to procure rest and act upon the skin, whilst it maintains the function of the secreting structures that pour their products into the alimentary canal, which must be carried off the succeeding morning.

In the stage of collapse stimuli and tonics must be conjoined or alternated with laxatives, and thus continued till the secretions become healthy.

The phenomena of autumnal fever point out the class of cathartics to be employed. The principal are calomel, colocynth, aloes, p. rhei, senna, ol. ricini, and spt. of turpentine. They are, however, for the most part used in combination. Professor Cooke's pill is a very excellent one, or a combination of calomel, colocynth, and tart. ant. will be found exceedingly valuable. The spt. of turpentine is admirably adapted to those cases on the wane, and when there is inflammation of the intestines.

The protochloride of mercury justly holds the highest rank. It more completely dislodges scybala and effectually corrects morbid secretion of the glands of the abdomen than any other cathartic. But I have never found it necessary—nor do I consider it safe in such *tremendous* doses as are recommended by some authors. The excessive use of this medicine frequently produces the most *shocking* consequences on the sonstitution, and renders life *abject and miserable.* I have seen patients labouring under high mercurial excitement, with a dry parched mouth, and sloughing of the cheeks and gums, and still some preparation of that metal was administered with the unsparing hand of empirical hardihood to relieve it. Appalling and unpardonable conduct! This is like giving a man cogniac to inebriate him, and then advise its free use to sober him again, or agreeably to the cant phraseology of the vulgar, "take the hair

of the dog to cure the bite." Forsooth, there are states of the system, in almost every species of fever, in which a benign salivation is considered, in our present state of medical knowledge, a sine qua non; if we fail to excite which the patient must die. Many congestive and inflammatory cases, in which the abstraction of blood is inadmissible, are directly in point. In such cases the appearance of ptyalism is generally ominous of the disease having succumbed; but this is not always the case. It is now a conceded point that the two diseases may co-exist—the primary outlive the mercurial affection, and at last prove fatal.

Having extended our remarks farther than we originally intended on this subject, we shall return from this digression and lay down the rules by which we are governed in administering cathartics.

1. They should be given on an empty stomach and in the remission, which is generally in the morning, so as to operate freely during the exacerbation.

2. They must be given in sufficiently large doses to procure *large* discharges of feculent bilious matter, and remove portal plethora, a desideratum in the treatment of fever.

3. Small doses are apt to produce small, frequent, or watery discharges, which are unfavourable, and should always be avoided.

4. They are to be repeated daily till local congestion and inflammation are relieved, and the biliary secretions become healthy.

5. After the fever is subdued, if the bowels continue in a soluble state, and the secretions of the chylopoetic organs become healthful, the appetite and powers of digestion soon return. But if the bowels are inclined to be sluggish, and the appetite weak, aperient medicine should be given to procure one or two consistent evacuations in the course of the day, and continued till the bowels become regular, the appetite keen, and digestion easy.

Affusions and ablutions. In ordinary cases of autumnal fever, as already observed, emetics, cathartics, under doses of tart. ant. and free ventilation, will produce a solution of the disease.

In complicated cases the lancet is to be employed. When the skin continues preternaturally hot and the secretions are perverted, if not suspended, and venesection is not admissible, cold affusions may be resorted to with unquestionable benefit; and repeated four or five times, if necessary, in twenty-four hours. If there be cerebral plethora, or a phrenitic tendency cold applications should be incessantly applied to the head. Ice in such cases is frequently resorted to. Pediluvia and rubefacients, applied for the purpose of revulsion, are also necessary. Cold affusions remove preternatural heat of the skin, reduce the force and frequency of the pulse, diminish headach, and restore healthy cutaneous function. They may be repeated whenever the skin is dry and hot; and are often useful in the advanced stages of fever. In the typhoid stage, when the debility and exhaustion are great, and the heat of the body is partial, sponging with tepid water and vinegar or whiskey will be preferable to cold affusions, under which the vital powers might not react. When there are frequent irregular exacerbations, or local determinations, or inflammation of the abdominal or thoracic viscera, tepid ablutions are more proper. Their sedative and soothing effect over the system is very manifest.

In congestive cases the warm bath, with other means to produce external excitation are necessary, and if opportunely applied are of great value.

When the feet are inclined to become cold, they may be alternately immersed in cold and hot salt-water. Thus the circulation and heat are invited into these parts.

The use of calomel does not prohibit the employment of cold affusions. They are employed with the happiest effects as a tonic during convalescence.

Sudorifics.—After sufficient depletion of the vascular system and alimentary canal, sudorifics are valuable auxiliaries in the treatment of fever. But means to extort diaphoresis should never be resorted to. The antimonial preparations, Dover's powder, and sal tartar. are among the most useful.

Blisters and rubefacients.—These are applied to obviate or relieve local congestions or inflammation that occur in the course

of fever. They should be preceded by venesection, antimonials and purgatives. When these means fail to afford relief, epispastics must be applied over the affected organ; and the functions of secretion and excretion maintained by alterants and laxatives till the disease succumbs. When the brain is affected, they should be applied first to the inferior extremities, which, in such cases, are generally cold; then to the nape of the neck, and lastly to the scalp.

In the low stages of fever, when the powers of life are flagging, and the stage of collapse is threatened, it must be prevented, if possible, by blisters to the ankles and wrists, sinapisms, sulphate of quinine, opium, vol. alkali, &c. If there be local determinations to any of the thoracic or abdominal viscera, rubefacients are frequently useful; and in highly congestive cases they are preferable to blisters. The latter are, in such cases, apt to be followed by sloughing of the parts blistered, which may prove exceedingly painful and dangerous to the patient, and a source of great anxiety and vexation to the practitioner.

Stimulants and tonics.—In the first stages of the congestive form of autumnal fever, stimulants cautiously used are not only admissible but salutary. It is however an egregious mistake to administer them in such prodigious and colossal doses as some physicians are in the habit of doing. And when they are administered with a view of obviating or anticipating the first symptoms of collapse, they should be employed with great skill and judgment. If they are used too soon they frequently kindle an inflammatory action in some vital organ, and then it is that the most appalling combination of debility and nervous excitement is seen for a short time before death. When the stage of collapse has come on, stimuli are peculiarly beneficial, and must be proportioned to the exigencies of the case. They are to be applied internally and externally; and the skin, liver and bowels must be synchronously acted upon by their specific stimulants. If the patient improves under the influence of stimuli it is an indication they may be continued with advantage; if he does not they must be with-drawn; and it is always improper to attempt to accelerate his recovery by augmenting

the quantity. In this stage the patient is frequently restless and distressed for the want of sleep. Denarcotized opium combined with camphor and ipecac have the best effect in composing the patient and exciting perspiration. Through the day the bowels must be gently moved by laxatives and enemata. Sulphate of quinine is one of the most potent remedies in this stage of the disease. I have given it in 2 gr. doses every hour to sustain the sinking powers of life, and in this way promoted a free sudatory discharge, which formed a crisis. Nor have I any hesitation in giving my assent to the use of this medicine in cases where there are remaining vestiges of inflammation that would prohibit the use of other tonics.

Fresh air in this stage is highly acceptable and refreshing. But atmospherical vicissitudes, and exposure to cold air in rising, must be avoided, because they may produce a fatal chill. Exposure even after convalescence, must be avoided; because a strong current of cold air in this debilitated state will almost invariably produce visceral perturbation.

The skin consists of an infinite number of vessels, and nerves, and hence is of vital importance to the general system. The dependance of function between the skin and liver, respiratory apparatus, intestines, &c. is well understood by the profession. If it fail to perform its normal office, some other viscus participates in the derangement—assumes a vicarious function. It is therefore of infinite moment to maintain uniformity of heat and healthy action on the periphery during convalescence. The circulating system is in a vacillating condition and the cutaneous vessels are greatly debilitated. Hence the external impression is easily communicated to central organs.

Diet.—It must consist of digestible and nutritive articles, such as milk, chocolate, Irish potatoes, lamb, venison, stale bread or crackers, &c. though these must be taken in moderate quantity, because if the stomach is overloaded the secerning vessels fail to perform their healthy office, and the whole animal machine is thrown nto disturbance in consequence of it. Diet as the natural stimulant and supporter of life is now mainly relied upon to recruit lost flesh and strength.

ART. VI.—*Cases of Clinical Medicine, which occurred in the Louisville Hospital.* By JOHN P. HARRISON, M. D.

THE Louisville Hospital was founded by the munificence of the Kentucky Legislature. The sources of its revenue are a duty of two per cent. on all the auction sales of this city, and a limited appropriation from the general government of the Union to the amount of expense incurred by the relief afforded seamen. The edifice is lofty, of spacious dimensions, and well ventilated by a judicious arrangement of its wards. The main central building of the pile is four stories high, with two wings of three stories each. There are four ample wards of about one hundred and fifty feet by thirty, with eight rooms of good size for the reception of patients. Three other rooms in the second story of the central building are occupied by the Steward's family, and another on the same floor is an apothecary's shop. A resident physician and a student of medicine live in the house. Three physicians and three surgeons attend the institution, for which they receive no compensation. The number of patients brought into the house varies with the season of the year. The greatest influx is in July, August, and September; there are, comparatively, a small number in the winter. The character of the acute complaints with which its inmates are afflicted is bilious fever; with its congeners, dysentery, &c. of a violent type, commonly contracted either on the Mississippi or Ohio, whilst engaged in the steam-boat navigation of these rivers. The diseases in spring and winter are usually phlegmasial; such as pneumonia, rheumatism, &c. Among the chronic affections we have to combat dropsies, hepatic complaints, pulmonary affections, &c.

The following cases I transcribed from my case book whilst in attendance on the hospital. They possess distinct features of practical interest, and may assist in the formation of a sound, en-

lightened judgment on some very important points in pathology and therapeutics. I shall give them with but few comments, my object being not to establish any theoretic views, but to impart clinical truths: for the propagation of brilliant fancies is at best a deceitful illusion to both him who gives and him who receives the splendid offspring of a mind ingeniously misemployed.

Case 1st.—Hodgkisson was sent to the hospital December 16th, 1826, with every manifestation of apoplexy. Symptoms, a few hours after admission, were great abdominal distention from retention of urine since his attack, which was several days anterior to admission; breathing stertorous; right pupil dilated, left pupil contracted; pulse full, somewhat compressible, and of regular rhythm, being ninety strokes a minute. He has been bled and purged, but the state of the bladder was overlooked by his medical attendants. The catheter was introduced after some difficulty, from the great elevation of the bladder over the brim of the pelvis.

The head was shaved and cold applications used to the scalp. He was bled a pint in the morning,—there being no mitigation of symptoms in the afternoon, the temporal artery was opened and twelve ounces detracted. Respiration was now occasionally suspended, and then resumed by sighing. He died that night. The examination after death revealed an effusion of blood on the surface of the brain on the left hemisphere near the longitudinal sinus, and under the middle portion of the parietal bone. At least an ounce of blood was effused from the ruptured vessel. There was a sanguineous extravasation in the surface of the brain beneath the place occupied by the effusion —this extravasation penetrated to the depth of an inch, and was about an inch in circumference. A small quantity of blood was found effused on the medulla oblongata. All the other viscera were sound. This man was intemperate to an excess seldom witnessed.

Case 2d.—Thomas Priestly came in December 19th, 1829. Disease, great hepatic enlargement very perceptible on the most superficial touch—the liver engrossed the almost entire abdominal and pelvic cavities. The skin was jaundiced very

intensely; there was cough with slight inky-coloured expectoration; diarrhœa, stools ash-coloured. He took a few doses of calomel and opium with little or no benefit, and died in two or three days. When he was opened the liver was removed and weighed;—its weight was upwards of ten pounds, avoirdupois. Its substance was altered in aspect; when looked at by a magnifying glass the structural arrangement of its acini had undergone a palpable mutation—the bile seems to have been secreted to a partial extent but was absorbed altogether by the veins—there was no appearance of that fluid either in the intestines, gall bladder or any of the ducts. The gall bladder was filled with a ropy mucous secretion. The increased magnitude of the viscus was owing to the deposition of a pale white substance, which some authors consider a deposition of coagulable lymph, whilst others regard it as partaking of an adipose nature. The ducts were all pervious. The veins about the verge of the anus were greatly distended, and just before death a copious eruption of blood had taken place from them. The fibrine of the blood was universally tinctured with yellow; none of the textures, except the skin, and some parts of the cellular tissue, were coloured with the icteritious hue. This poor fellow had been sick with bilious fever, and fever and ague, some months on one of the steam-boats, and very little attention had been paid him.

Case 3d.—Charles Memer, a thin young man of a sunken dejected countenance, came under treatment March 14th, 1827. He had been very ill with bilious fever last fall in Natchez; after his partial restoration to health, he was affected with failure of appetite, retarded action of the bowels, and general lassitude after slight exertion. Whilst ill with fever he had been salivated, and now one of the disciples of Serapion in a small town on the Mississippi persuaded him that a portion of his liver was mortified, and must be taken off by administering nitric acid. One ounce of acid was diluted with five ounces of water, of which an ounce was to be taken at a dose. The first dose literally cauterized his throat, and he forthwith abandoned this rash prescription. His stomach felt the injury thus inflicted,

and sub-acute gastritis of the organ resulted. Several months elapsed before his application for admission into the Louisville Hospital, during which period his digestive energies were nearly annihilated: the blandest nourishment was with difficulty converted into chyle. So difficult was it for the prostrate, enfeebled organ to subact the materials submitted to its peptic process. Lime water and milk agreed best with his stomach, and by this he was mainly sustained in a frail and feeble state of being.

His case presented a group of symptoms at once melancholy and hopeless. His sleep was disturbed with distressing dreams, his mind in its wakeful thoughts was tossed on a sea of unquiet and gloomy anxiety, "filled with thick coming fancies" of deep and hopeless despair; his sensations after eating were distressing in the extreme, head-ach, confusion of thought, præcordial oppression, and a creeping sensation of a disagreeable kind coming over the entire surface, radiating from the stomach. His tongue was clean to a remarkable degree, but rather red, his thirst was not very great, his bowels were obstinately bent on inaction, the surface of the body was dry, his pulse not more than fifty strokes a minute, and very weak, and his indisposition to muscular effort so great as to chain him in mechanical fixedness to his bed, though he had no fever, and his remaining strength was sufficient for locomotion. There was considerable tenderness felt when pressure was made on the stomach, but no urgent pain; there was great power of retentiveness in the stomach, the articles received never being ejected. The colour of the alvine discharges was natural, but they were indurated from long retention in the bowels, except when produced by cathartic medicines, and then they were thin and yellow. There was no pain in the region of the liver, though frequently he complained of a pain at the top of the right shoulder. His breathing was affected at times after taking food, and then oppression at the heart was realized. There was no sensible induration or enlargement of any of the abdominal viscera. After a consultation held by the physicians of the hospital on his case, we determined to give the blue pill every other night in doses of eight

or ten grains with a drachm of charcoal three times a day, and apply a blister over the stomach. The mercurial oxide created soreness about the mouth, with an enlargement of the cervical glands, and its employment was soon abandoned. His diet was to be of solid aliment, such as rice, a little mutton, stale bread, &c. Some of the faculty considered it gastritis, others thought that there was ulceration of the mucous tissue of the stomach. But I thought there was more evidence of atony and nervous irritability about the case than of inflammatory action in any important organ. Upon this view of the case I grounded my subsequent treatment. At the commencement of the treatment I was regulated by other considerations than those just enumerated. Every kind of liquid aliment distressed him to a degree never seen before by myself. Soups, &c. were consequently interdicted. Acidity of the primæ viæ annoyed him after any kind of aliment was taken. After employing a variety of remediate measures, such as friction, warm salt bath, enemata, soda, bismuth, carbonate of iron, turpentine, gentle emetics, blue pill, charcoal, I settled down on the following plan, which was more satisfactory in its effects than all the others. A permanent issue was established on each rectus muscle, just above the umbilicus, the bowels were kept open by tepid and copious injections, exercise was enjoined, frictions were used on the extremities, the muriate of lime was given three times a day, beginning with twenty and increasing it gradually to thirty drops of the saturated solution in water, and the diet was milk, with soda in it, so as to prevent too quick a chemical change of the milk by morbid acid in the stomach, mutton or young chicken broiled, with dry stale bread, &c. He remained in the house upwards of four months, at the end of which time he was much improved, his digestive energies had in a great degree been restored, his mental hilarity was returning, and his muscular power was seen by his taking enjoined exercise in the ward and about the yard. In consequence of some disagreement with the Steward he left the house in dudgeon, and I did not see him till early the ensuing winter. My surprise was very great when I met him in the street in a high state of *en bon point*, and with an animated ex-

pression of countenance. Subsequent to leaving the hospital he had betaken himself to exertion, drank grog pretty freely, and lived as he listed on whatever came first; his bowels were at times disposed to costiveness, but nothing in comparison to what their condition was formerly. The stomach was no longer that inert, querulous and fastidious thing it had been, but had become a decided and energetic advocate of the rights of the economy and a faithful fulfiller of its functions.

More than a year after his leaving the hospital, he was brought back under a most alarming mercurial ptyalism, utterly unable to speak, there being great tumefaction about the mouth and throat. Suppuration took place under the lower jaw-bone, and he soon died. No organic lesion, not even the least trace of any former structural alteration, was to be disovered in any of the viscera, though they were examined under an expectation of finding some vestige of former organic change.

Case 4th.—White Coxy, affected with phthisis pulmonalis of six or eight months duration. There was nothing worth a particular notice in this case from any peculiarity, discoverable by dissection, except the carious condition of the petrous portion of both temporal bones. He was entirely deaf before death, and previously to becoming so, he suffered severe agony from the earache, aud had purulent fetid discharge from both ears. There was an entire destruction of the mechanism of hearing in both ears—there was no tympanum—no small bones—no infundibulum, cochlea, and semicircular canals—all were destroyed—a probe introduced at the meatus auditorius externus passed without impediment to the dura mater, which was becoming diseased, with the portion of brain lying over the carious bone. The lungs were extensively tuberculated, and the mucous coat of the small intestines was ulcerated.

Case 5th.—Matthew Revel, native of Ireland aged 22 years, came under my care April 11th, 1827. He has been sick seven weeks with ague and fever. There is ascites with anasarca of the face and feet: he cannot lie on the left side: spleen much enlarged; has diarrhœa, with loss of appetite; pulse moderately full and strong. He was purged with crem. tart. and

jalap; had calomel given as an alterant. After he had been in the hospital several days, discovered in examining his thorax some distinct evidences of hydrothorax—has now some cough. He was purged, blistered, took calomel, digitalis, squills and crem. tart. alternately, but no visible amendment resulted from the treatment. On the 4th of May he died. Upon examination a few hours after death, the following appearances of disease were seen. The abdominal cavity contained a quart of water, and more than a pint of thick gelatinous substance, of the consistence of paste, or starch as prepared for starching linen: the peritoneum was thickened generally; the colon and stomach were adherent; a firm and general adhesion existed between the heart and pericardium: the lungs were adherent to the pleura costalis of both cavities; the right lung was partially hepatized, and was immersed in a pint of serous fluid.

ART. VII.—*Singular Case of Artificial Anus, successfully treated.* By GEORGE W. CAMPBELL, M. D. of Columbia, Tennessee.

ABOUT the 1st of October, 1828, Doctor Anderson, of Giles county, was called to see J. Jones, Esq. aged about 40, inclined to be corpulent; found him complaining of great uneasiness throughout the abdominal viscera generally, but mostly of a heavy dragging kind of pain in the right side, with sensation of fulness there. He had little or no fever, but was very much alarmed, owing to the unusual feeling in the bowels.

Being attacked in the night, after taking a hearty supper, his case was thought to be one of colick. Cathartic medicines of a mercurial kind were prescribed, with directions that enemata should be used if the bowels were not operated upon. The purgative medicines were repeated frequently, with bleeding, anodynes, &c. but it was not until after a lapse of three or four

day's perseverance in this treatment, that a passage was effected through the bowels. After the bowels were pretty well operated upon, the irritability of the stomach, together with the excruciating pains of the bowels, measurably subsided, leaving a dull heavy pain, with some fulness in the right hypochondrium. Blisters were applied frequently over the swelled part, and purgatives were continued for two weeks; at the end of which time he was thought to be some better: he was then directed to use nothing but light aperient medicines. There appeared to be but little alteration for ten days, when it was ascertained that his discharges had become of a pale colour. This gave rise to a suspicion that the liver was implicated; for which an alterative course, with calomel, was commenced and kept up for several weeks, without any other effect than that the discharges became of a natural appearance.

During this time the seat of uneasiness gradually fell down into the right iliac region, where it remained permanent for a few weeks, when a swelling, with the appearance and symptoms of a deep seated phlegmon, appeared on the back, just below the right kidney and the last rib. Blisters, &c. were frequently applied without benefit. On a minute examination of the tumor, there was discovered fluctuation, with a gurgling sound. An incision was made into the swelling with a scalpel, which was followed by a discharge of some bloody water and a little pus, with a puff of wind. Being very much reduced and weak, tonics were directed and continued for some time, but did no good. His recovery was now almost entirely despaired of by himself and friends; when dressing the ulcer, which character it had by this time assumed, a body was seen to occupy the bottom of the ulcer, which was laid hold of with a pair of forceps and drawn out. The doctor, together with two other physicians who had been called in consultation, supposed the substance to be the result of a sloughing of the muscles of the back, as the disease was immediately over the sacro-lumbalis and longissimus dorsi.

There was no discharge following the substance, when drawn out; but about six hours afterwards, the natural contents of the

bowels were discharged at the opening. The alarm now became greater and the last hope, as the patient supposed, was in an operation, to sew up the bowels from whence the discharge came; for which purpose I was sent for on the 2d of January.

I found him reduced almost to a skeleton, by three months confinement with severe illness; with great anxiety and dread of the operation which he expected to undergo, and the awful apprehensions of death. The dressings were removed from the part, and the first thing that attracted my atention, was the discharge of chyle and bilious matter, which appeared to be continually escaping from the opening, and had excoriated the skin in the neighbourhood of the place. I now felt pretty certain that the duodenum, or some portion of intestine not far remote from it, was involved in the slough that had taken place; and that a portion of intestine remote from the stomach and duodenum was also involved. This opinion was founded on the circumstance of the constant discharge of chyle, &c. which must have come from the duodenum, or some portion of bowel near to it; and secondly, upon there being, occasionally, discharges of fecal matter which must have come from a portion of the intestine, remote, physiologically, from the duodenum.

The opening had not been examined previous to my arrival, except with a small probe, which could have afforded but little information respecting the situation of affairs within the cavity of the abdomen. I now introduced my finger into the opening; and the first thing, after getting through the skin, muscles, &c. (and they were soon passed, as the swelling that had previously existed, had entirely subsided, and left rather a depression in the neighbourhood, and as the system was so very much reduced and emaciated,) that attracted notice, was the lower part of the kidney, which could be felt distinctly; the psoas magnus muscle, which gave the finger a direction upward and forward and brought it in contact with the liver, which appeared to have been somewhat enlarged and extended lower down than common. I could not distinguish, precisely, the colon and duodenum, from other substances that presented themselves, but I felt confident that the duodenum was somewhat drawn

down from its natural position, together with a portion of colon and omentum. I could easily introduce a bougie, upward and forward, into the duodenum and stomach; into the colon, in the direction it takes from the liver, forming its transverse arch, and in the opposite direction, as it ascends from the right iliac region.

After this examination, I was satisfied, as I had previously expressed a belief, that the rupture, or loss of substance, in the duodenum and colon, was confined to those portions of them which were in contact with the peritoneal covering of the abdomen and with themselves; or such parts of them as had no peritoneal covering, and that such adhesions had taken place between the duodenum, colon, omentum and peritoneum, as prevented the contents of the bowels from escaping into the common cavity of the abdomen.

When I had satisfied myself as to the true nature of his situation, I told the patient, that there was no necessity for any operation, and that I should make none; that nature had anticipated me in the operation, the performance of which, she commenced, with the commencement of his illness, and that his situation, at present, showed, that nature had performed the operation, thus far, more completely and more successfully, than it could possibly have been done, by me, or any other man; and that he would certainly get well. At this he expressed dissatisfaction, by saying, "after sending thirty miles for me to come to cut him open and sew his bowels together, that I now refused to do it, and he was left to die, without having any thing done to save him." After being made acquainted with his true situation however, he abandoned all idea of an operation and consented to what course was thought most proper.

He was directed to have the opening injected, occasionally, with a decoction of oak bark; to take three or four quinine pills a day, and have the bowels kept open, principally, by the use of enemata. A compress and bandage were applied with a view to prevent the contents of the bowels from passing out, and to facilitate cicatrization. There were no farther discharges of

feces; the ulcer soon healed up; he gained strength rapidly, and is now perfectly well.

I examined the substance which came out of the artificial opening, which filled a common sized plate, and found it to consist principally, of omentum and portions of intestine.

As to the true condition of the parts involved in this disease, within the cavity of the abdomen, we cannot arrive at certainty; but taking all things relative to the case into consideration, I am strongly inclined to the belief, that there was intussusception. In all probability, the cœcum, with a portion of ilium, was inverted into the ascending colon, carried up in contact with the liver, duodenum, &c. and formed the swelling and hardness, which were present in the right side, from the commencement, until the abscess was formed on the back; which terminated in inflammation, sloughing, and loss of omentum, duodenum, colon, the whole of the cœcum, and a portion of ilium.

Whether this invagination of the intestines, was produced by the efforts which were made to remove constipation &c., or whether it existed at the commencement, and was the cause of constipation, pain &c. I am not able to say; but I am much in favour of the opinion that these effects may, and are produced from the repetition of powerfully drastic and irritating purgative medicines, that are so frequently resorted to, with a view to break down an energy that is acting in opposition to the healthy peristaltic motion of the intestines. And between the operation of the two opposing causes, or principles, I can conceive, that intestinal invaginations may readily take place. One portion of intestine, may be under the influence of increased peristaltic motion, while another portion is under the influence of violent antiperistaltic motion. I can conceive also, that anodynes, unskilfully managed, may be a cause of intussusception. When the bowels are in great commotion, whether from disease or the attempts to remove it, anodynes may be partial in their effects; while they allay the disturbance in one quarter, they produce no effect in another. The calibre of the bowels, may also undergo irregularities, from the effects of disease, or unsuccessful attempts to remove it. While some parts are con-

tracted by some spasmodic action, other parts are distended by flatus, which would constitute an additional facility to the formation of invaginations.

I have at this time, under my care, a boy, aged 14, who has been labouring three months, under disease very similar to that in the case which I have just mentioned. When he dies, which I suspect will be very shortly, a post-obit examination, will render a history of his case interesting and instructing.

ART. VIII.—*Cases illustrative of the utilities of the Bandage.* By THOMAS J. CATCHING, M. D. of Georgetown, Mississippi.

(COMMUNICATED IN A LETTER TO PROFESSOR DUDLEY.)

Case 1st.—Dr. W. P. Harris and myself visited a negro man who, from the fall of a tree, had suffered violent concussion of the brain, together with a fracture of the right femur about its centre. Four or five hours had elapsed after the injury before we saw him. He was still lying perfectly insensible. After venesection we proceeded to apply the bandage, as firmly as I could draw it, to the injured limb. The negro having manifested some signs of recovery from the cerebral injury, the venesection was repeated, and a cathartic given. We then left him till the next morning, when we found his thigh, which, when the bandage was put on, was enormously swollen, almost as small as the uninjured one. We directed his bowels to be opened every day; readjusted the bandage; and directed it to be tightened as often as it should become loose from the reduction of the swelling. In this case, although the general system was much disturbed by the injury of the head, yet the patient recovered speedily, without a single unpleasant symptom as the effect of the roller.

Case 2d.—A negro boy in falling from a horse received a fracture of the left femur about its centre; a fracture of the ulna near the carpus; and a dislocation of the carpal joint. I saw him in the course of fifteen minutes after the injury, and immediately replaced the bones and applied the bandage to the injured limb very firmly. He complained of scarcely any pain. No swelling took place. He was well in a few days.

Case 3rd.—A little boy, the son of Mr. B. aged about 6 years, was brought to me with a dislocation of the upper extremity of the radius. I reduced the luxation, and applied the bandage. The little patient got well, having suffered but very little pain.

Case 4th.—Mr. S. who had laboured under a disease of his ankle joint for five years, applied to me. An extensive sinus was formed which discharged itself just behind the malleolus externus. The probe could be inserted between almost all the bones of the tarsus; and the matter discharged gave evident signs that the bones were slightly affected. I directed him to take five grains of blue pill every night on going to bed; to regulate his diet, and to avoid exercise on his diseased limb. I then applied the bandage as firmly as I could, according to your directions. In three or four days his ankle was much reduced in size; the discharge from the sinus was much altered in colour and smell, and considerably diminished. By pursuing this course three weeks, his ankle was entirely well.

Case 5th.—In a case of Phlegmasia dolens where both the inferior extremities had attained to an enormous size, I adopted a constitutional treatment, and put on the bandage with the most happy effect; the swelling disappearing speedily, and the lady regaining her health in two or three weeks.

Case 6th.—A child was brought to me with aneurism by anastomosis seated about the centre of the right parietal bone. I applied the bandage and compress with the most complete success; the circulation in that part of the scalp becoming healthy in a few days.

I have treated many cases of paronychia with the bandage; and with the most complete success. Several cases of ulceration of the legs have come under my observation, in which the bandage with a constitutional treatment has succeeded in a short time. I have witnessed the practice of several other physicians, and I have never known any unpleasant symptoms from the application of the bandage, when it has been so applied as to produce equal pressure on every part of the limb; and when the antiphlogistic regimen has been prescribed by the physician and pursued by the patient. On the contrary it has always been attended with the most complete success.

ART. IX.—*Singular Case of Rheumatism.* By Dr. James Kelly, of Jonesborough, Jefferson county, Alabama.

(COMMUNICATED IN A LETTER TO PROFESSOR COOKE.)

Dear Sir, I have suffered long, and severely, with rheumatism of a peculiar character, and feel some desire that my case should be published. If you should think the case merits publication, please give it a place in the Transylvania Journal of Medicine &c.

Case.—In the fall of 1825 I was taken with a pain in the tendo-Achilles of my right leg. It caused me to halt very much in walking, as I could not bear the least flexion of my foot without great pain, but did not incommode me when at rest. I paid but little attention to it as my general health was not affected. In two or three months it left this situation, rose, and fixed itself in the joints of my jaw. Here it was not painful except when I attempted to open my mouth, which I could not do without inducing considerable pain. I consequently could not masticate my food and was compelled to live on spoon victuals. My general health, however, remained unaffected. I applied spt. turpentine and a variety of liniments, with flannel to my jaw, but I could not discover that they did me any good. In a few months however, viz. in the spring of 1826, the pain voluntarily left the joints of my jaw and forthwith located itself in my right arm and shoulder. Now the character of the disease changed —my whole arm and shoulder became most distressingly painful. The pain was decidedly worse when my arm and shoulder were at perfect rest, and my best mitigating remedy was to keep them in rapid exercise; either by swinging, friction, or percussion. I tried many liniments, embrocations, and the bandage, but they did me no good. Many a sleepless night did I spend walking my room, swinging my arm.

During this summer, and indeed for several summers before, I had repeated attacks of bilious fever, and underwent active

treatment by bleeding, emetics and cathartics. It all however gave no permanent relief to my arm and shoulder. My arm shrunk, and I pretty much lost the use of it, while my general health began to suffer permanently. After suffering with my arm and shoulder about six months, and after having tried a great variety of remedies, I was speedily relieved by the action of a very large blister, covering my arm all over. I felt rejoiced in the hope, that I was relieved of this excruciating affection, but alas, so soon as it left my arm and shoulder, it descended a little, and took up quarters in my right hip and thigh, and here it has maintained its ground, with almost unconquerable obstinacy, and inflicted sufferings the most cruel and insupportable. It was in the fall of 1826, that it commenced its ravages in my hip and thigh, and no subsequent metastasis has taken place. The sacro-iliac junction, the parts immediately about the trochanters, and all the anterior muscles, and fasciæ of the thigh were the particular seats of disease. The thigh perished away to little more than half the size of the other; and I became permanently lame, often having to use crutches. The disease now became more decidedly paroxysmal. A spell would last from four to twelve weeks. I would then have a remission for a few weeks; when all the former scene of suffering would again be renewed. During a paroxysm, sleep was out of the question. I think I have several times, spent from four to twelve weeks, without enjoying sound repose for one hour at a time. Night after night, and day after day would pass away, while I would continue moaning, screaming and tossing in my bed. To mitigate the pain, I was often compelled for hours at a time, to keep some person, pulling, rubbing, and rolling my thigh. My general health now suffered very much: my appetite bad, my flesh and strength gone, my skin pale and sallow. During the winter and spring of 1827 a great variety of remedies were tried with but little beneficial effect, and that little only temporary.

Recollecting that a blister had relieved my arm, I enveloped my thigh in a plaister extending from below the knee, up the whole length of the thigh and round to the sacro-iliac junction. It drew well and produced a most severe and dangerous stran-

gury. The pain was of course relieved for a time, but as soon as the blister began to heal, the pain began to return, and in a short time was as bad as ever. I now felt very much disheartened, and frequently invoked the God of Heaven that I might speedily die or get well. Opium would not allay my sufferings even temporarily, and always did me injury. A full dose of tartar emetic would always give me immediate, but only temporary relief. On taking a full dose of tartar-emetic I have often fallen asleep in five minutes, when before swallowing it I was in the utmost agony. After vomiting freely I would remain better for several hours, sometimes several days. This induced me to take it frequently, and I judge I have taken two or three dozen strong doses for the sake of this temporary benefit. I also took strong cathartics of gamboge and calomel, but they did not benefit me much; for purgatives always operated with me very imperfectly. I tried salts and other purgatives but to no better purpose. My liver seemed particularly sluggish in action, for neither would the emetics or purgatives produce much evacuation of bilious matter.

A symptom a little peculiar, and that I have not yet mentioned, was a sore spot on the scalp about as large as a dollar. It was situated about the middle of the left parietal bone, and was so sore as to give me considerable uneasiness, at times, in wearing my hat. It appeared early in the disease and continued throughout its whole course. It sometimes spread and affected my eye, but generally remained in a circular spot as above described.

In June 1827 I visited the Blount springs (of this state) and they relieved me almost immediately. They operated as a laxative and powerful diuretic. I remained there about a month; when feeling quite well, and my business calling me home I left them.

Again my spirits were elated with the hope, that I was finally well of this most dreadful disease. On engaging however in the duties of my profession, which called me out in bad weather, the disease soon returned.

It was in the fall of this year (viz. 1827) that I saw Doctor

Hereford's communication in the 40th number of the Medical Recorder, in which he stated that the capsicum had relieved him of a rheumatic affection. I was at that time suffering prodigiously, and immediately had a strong decoction of it prepared, and drank most copiously of it. It gave me speedy and great relief; but its effects, like every thing else that I had tried, were temporary only. Another spell soon came on, and though the capsicum did in some degree mitigate my sufferings, yet no quantity of it would give any thing like relief. I took it for several months, but finally abandoned it. I was now advised to try Swaim's panacea; which I did, (for a drowning man will catch at a straw) but it did me no good. Thus it seemed that I was doomed to drag out a miserable existence, neither being able to live in the least comfort, nor to die. So I continued until August 1828, a period of nearly three years since the commencement of my sufferings, when I had an unusually severe and protracted spell.

I determined that I would continue trying to cure myself as long as I lived; for it is not a disease that will permit you cheerfully to resign yourself to the bed, and contentedly await the approach of death—it is too severe.

I had for a long time believed that my disease depended upon a slight functional derangement of the liver. My principal reason for this belief was, a small deficiency of bile in the alvine evacuations, thereby rendering them rather too light in appearance. This deficiency was generally much more apparent for a few days before an attack, and throughout the paroxysm. My tongue was always covered with a white fur—slight mental despondency generally existed, but was increased considerably for a few days before an attack. Indeed it seemed to go pari passu with the biliary secretion. I however, never had any pain or uneasiness in the liver in my life. My bowels were not costive, nor in any way irregular. Acting on the belief that the liver was the original seat of disease, I had often taken strong emetics and cathartics, as mentioned before, but without obtaining any permanent benefit. I had also once or twice, taken

calomel in grain doses, three times a day, until it slightly affected my mouth without any benefit.

Encouraged by your Theory of Fever, and the success of your treatment, which I think applicable to a great variety of diseases, I determined once more to try the effect of remedies directed to the liver. The following pills being ready prepared in my shop, and having prescribed them very often for five or six years in bilious fever and other diseases, and having often discovered that they produced the exact character of discharges, that you think so desirable, I concluded that they would answer as well as any.

℞ Calomel, 60 grs.
James's Antimonial Powder, 6 grs.
Opium. 3 grs.—to make a dozen pills.

I was as ill as I could be when I commenced taking these pills. I took two of them every four hours until I had taken them three days; when my discharges became black, consistent, and gelatinous, and I was relieved—my mouth was very slightly affected. I immediately felt an unusual flow of spirits, and continued to amend rapidly until my health became much better than it had been since the commencement of the disease. I have ever since taken two or three blue pills twice or thrice a week. I have had several slight attacks since, but never to confine me, or prevent me from sleep. I have always been able to repel the attacks speedily, by increasing the dose of the blue pill, or by resorting to the former preparation until I produced the dark gelatinous discharges as before. My health is now pretty good, though my leg has not entirely regained its strength, and it is sometimes a little painful; though it is so trifling that I should not notice it, if it were not for the recollection of former suffering. In fact the joy that I feel at the change in my situation is inexpressibly great. And I feel a degree of confidence bordering on buoyancy, that the plan of treatment will restore me to perfect health, and all others similarly situated.

Remarks.—That my case had some peculiarities I think will be admitted—at least they were so to me. 1st. Whilst the pain

was situated in my shoulder, arm, hip, and thigh, it was always decidedly worse when I was at perfect rest. My best mitigating remedies consisted in swinging, pulling, rubbing, and rolling my limbs. In all other cases of Rheumatism that I have seen, motion increased the pain. A second peculiarity was the sore spot on the scalp. How this was connected with the disease I cannot say; but this I know, that it existed pretty well throughout the whole term of my disease, and so soon as I obtained relief from the pills it left me, nor have I felt it since. I had several times shaved the place and blistered it, but it would not relieve it.

The disease never affected the left side of my body, except the sore spot on my head, and the left joint of my jaw; thus resembling hemiplegia.

I have thought that the brain was in some way, though secondarily implicated, in the chain of diseased action, and that the pains were the immediate offspring of this cerebral irritation.

I should be very glad to see your remarks on the case, for it would be highly gratifying to me to have a clear conception of its true pathology. I am afraid I have been too tedious in the relation of this case, but I could not well more abridge it, and still bring all its windings into view: and the subject must surely be considered of paramount importance.

I am your's respectfully,

JAMES KELLY.

PROFESSOR COOKE, Lexington, Kentucky.

ART. IX.—*Florula Lexingtoniensis, secundum florendi ætatem digesta; or a descriptive Catalogue of the Phænogamous plants indigenous to this portion of Kentucky; arranged in the order of their periods of flowering.* By CHARLES W. SHORT, M. D. &c.

FASCICULUS IV.

From the first to the middle of May.

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61. GENUS AQUILEGIA. *L.*

(*Deriv.* Latin, *aquila*, an eagle; the nectaries being fancied to resemble an eagle's claws.)

Class 12*th. POLYANDRIA—Order, DI-PENTAGYNIA.*

Gen. Ch. *Calyx* none. *Petals* 5. *Nectaries* 5, bearing spurs between the petals. *Capsules* 5, distinct.

Species 1*st.* AQUILEGIA CANADENSIS. *Wild Columbine.*

Sp. ch. *Spurs* straight; *styles* and *stamens* exserted; *flowers* pendulous; *leaves* ternate and bi-ternate, their segments 3-parted, obtuse at the summit, notched and toothed.

Obs. This, the only native species of the genus, is among the most beautiful and common of the North American plants, surpassing, in the delicacy of its leaves and brilliancy of its flowers, the common cultivated species, (*A. vulgaris*). The wild columbine is found throughout the Union, always preferring the rocky, moist banks of creeks and rivers, but thriving well when removed to different situations. It is readily recognized by its drooping flowers, of bright red and yellow colours, in which the alternate arrangement of 5 petals, and 5 hollow, spurred nectaries, constitute the characteristic of the genus. These, after remaining for a considerable length of time, fall off and leave 5 distinct capsules, terminated by long styles and filled with numerous seed.

Flowers usually about the 1st of May: in favourable seasons somewhat earlier.

62. Genus. DODECATHEON. *L.*

(*Deriv.* Greek, *dodeka theoi*, twelve Gods; an old name of heathenish origin, taken from the twelve flowers, or thereabouts, comprising the umbel of the plant.)

Class 5th. PENTANDRIA—Order, MONOGYNIA.

Gen. ch. Calyx 5-cleft. *Corolla* rotate, reflected, 5-parted. *Filaments* very short; anthers connivent. *Capsule* 1-celled, oblong, opening at the apex.

Species 1st. Dodecatheon Meadia. *American Cowslip.*

Sp. ch. Leaves oblong, oval, (repandly toothed?) *umbel* many-flowered lax: *bractes* oval.

Obs. I have been disposed to question the accuracy of those who have made two species of this genus; viz. *D. Meadia* and *D. integrifolium*; the characteristics of the latter being, according to Michaux, who is followed by Pursh, Nuttall and Torrey, "leaves oblong-oval, subspathulate, entire; umbel few flowered, straight; bractes linear,"—"a much smaller plant than the preceding;" and Dr. Torrey gives 4 inches as the ordinary length of the leaves of *D. Meadia*. A plant before me, selected as a medium specimen from many others found on the Kentucky river, has leaves 10 inches long, and more than 2 inches broad, which are perfectly entire on the edges; the scape being about 1 foot in height, and from 12 to 14-flowered. The leaves of this specimen would consequently point it out as a luxuriant growth of *D. integrifolium*, whilst its very size, the number of flowers, and their drooping habit would designate it as *D. Meadia*. I conclude consequently that they are but varieties of the same species, differing in appearance from soil, situation and exposure.

The plant in question is among the most interesting of our Flora. Like the Columbine, in whose company it is often found, it prefers the moist and shaded hill-sides bordering water-courses; and is strikingly characterized by its tuft of smooth radical leaves; its naked stem, bearing at the summit, on long pendent peduncles an umbell of white flowers, sometimes tinged with delicate purple, their petals reflected and twisted, marked in the centre with a brown areola, which is tinged exteriorly with yellow. These successively evolving, make a beautiful show, and continue for some time in bloom. Flowers about the 1st of May.

PHACELIA. (See Genus 36.)

Species 2nd. Phacelia bipinnatifida.

Sp. ch. *Stem* erect; *leaves* pinnatifid; segments incisely lobed; *racemes* mostly bifid, oblong, many-flowered; divisions of the *corolla* entire.

Obs. This plant is also an inhabitant of the cliffs and precipitous, rocky banks of the Kentucky river and other water-courses in this neighbourhood. It is distinguished from the first mentioned species (*P. fimbriata,*) by its more luxuriant growth, deeper blue flowers, which are not fringed, and by a peculiar, strong, resinous odour emitted by the plant, particularly when the seeds are maturing. At this stage of its growth, from the elongation of the racemes, which are thickly beset with capsules, it assumes a diffuse, procumbent habit. Flowers 1st of May.

The genus *Phacelia* seems to have been unnecessarily severed from *Hydrophyllum*; and the present individual approximates still nearer to that genus in the bearded character of its stamens.

63. Genus. HYDROPHYLLUM. *L.*

(*Deriv.* Greek, *hydor,* water, and *phyllon* a leaf—water-leaf.)

Class 5th. PENTANDRIA.—Order, MONOGYNIA.

Gen. ch. *Corolla* campanulate 5-cleft, with five internal, longitudinal, nectariferous grooves; *stamens* exserted, filaments bearded in the middle, *capsule* 1-celled, 2-valved, 4-seeded, with 3 of the seed abortive.

Species 1st. Hydrophyllum Canadense. *Shawnee salad.*

Sp. ch. *Leaves* simple, lobed, angular; *flowers* in crowded fascicles.

Obs. Root leaves on long petioles, large and broad, somewhat palmate, 5-7 lobed, cut and toothed. Stem leaves smaller and on shorter petioles; flowers white, pale blue, or purple, less densely crowded together than in the succeeding species. The plant is about 18 inches high when fully grown, and every where covered with an hairy pubescence, particularly on the calyx. The leaves are said to be eaten by the Indians as a salad. Common in half cultivated lands, about fence corners &c. Flowers about 5th of May.

Species 2nd. HYDROPHYLLUM APPENDICULATUM.

Sp. ch. Very hairy; *radical leaves* sub-pinnatifid; *cauline* ones angularly lobed; sinuses of the *calyx* with minute oval appendages.

Obs. This species of water-leaf is more commonly confined to the rocky banks of creeks and rivers, being distinguished, even when not in flower, by its large sub-pinnate, coarsely toothed, deep green leaves, which are marked with maculæ of paler green. From the tuft of root-leaves arise one or more stalks to the height of 12 or 18 inches, sparsely supplied with leaves, and garnished at the summit with one or more dense, globose heads of flowers: these like the preceding are of various shades of colour, from white to pale blue and purple. The whole plant is hairy, and especially the calyx. Flowers usually during the first week of May.

PHLOX. (SEE GENUS 33.)

Species 2nd. PHLOX SETACEA? *Ground Pink.*

Sp. ch. Cespitose, (pubescent?) *leaves* fasciculate, subulate, rigid, ciliate; *flowers* few, terminal, somewhat umbelled; segments of the *corolla* cuneate, emarginate; teeth of the *calyx* much shorter than the tube of the corolla.

Obs. I have ventured with doubt to affix this name (*setacea*) to the present species, although it agrees nearer with the description given above, than any other specific character to which I have access, and I can scarcely suppose it a new species. I have met with the plant but in a single locality, on the Kentucky river at the mouth of Hickman creek, where on the precipitous cliffs which over-hang the river, it forms a beautiful object during the month of May. Its root being fixed in the soil collected in the fissures of perpendicular cliffs, the main branches hang down to the length of 10 or 12 inches but are not stoloniferous, and from them arise numerous secondary branches about 4 inches long; these are thickly beset at their origin with stiff, narrow, smooth leaves of about 1 inch in length, which are shorter on the ramuli and become somewhat oval and ciliate. Each small stem supports from 1 to 8 flowers, which are white or pale blue throughout. These flowers, thus produced in immense numbers, form dense circular tufts on the surface of the rocks quite concealing the foliage of the plant. The whole plant is destitute of pubescence except the ciliæ of the smaller stem-leaves. Flowers about the first week of May.

64. Genus. SILENE. *L.* *Catchfly.*

(*Deriv.* Probably from the Greek *sialon* saliva; in allusion to the frothy, viscid moisture on the stalks.)

Class 10*th.* *DECANDRIA.—Order, TRIGYNIA.*

Gen. ch. *Calyx* cylindrical. *Petals* 5, clawed, crowned at the throat. *Capsule* 3-celled.

Species 1*st.* Silene Pennsylvanica. *Wild pink.*

Sp. ch. Viscidly pubescent; *radical leaves* cuneate; *stem leaves* lanceolate; *panicle* mostly trichotomous; *petals very obtuse.*

Obs. "A small semi-procumbent plant with large rose-coloured flowers." Several stems arise from a common root to the length of 6 or 9 inches, each bearing at the top, about three flowers which are quite handsome. The lower leaves, when the plant is in bloom, are for the most part yellow and decaying, those on the stem generally petiolated, sometimes almost connate. On the poorer, exposed hill-sides bordering the Kentucky river. Flowers about the 5th of May.

Species 2*nd.* Silene Virginica. *Crimson Catchfly.*

Sp. ch. Erect or partially decumbent, vicidly pubescent; *leaves* oblong lanceolate, scabrous on the margin; *panicle* dicotomous; *petals* bifid; *stamens* exserted.

Obs. A much taller plant than the preceding, more viscidly pubescent on the leaves and stems, and especially the calyx; it is moreover strikingly distinguished by its bright scarlet flowers, the petals of which are deeply bifid at their ends. The plant when fully grown is 1 or 2 feet high, branching towards the summit, each branch bearing from 1 to 3 flowers. Leaves opposite and of a dark green. Less common than the preceding, but found in similar situations; blooming somewhat later.

TRILLIUM. (See Genus 20.)

Species 2*nd.* Trillium erectum. *Upright Trillium.*

Sp. ch. *Peduncle* inclined; *flower* nodding; *petals* ovate, acuminate, flat, spreading, broader and a little longer than the calyx; *leaves* broad rhomboid, acuminate, sessile.

Obs. A much larger plant than the first mentioned species, (*T. sessile.*) stem 12 or 18 inches high. Peduncle inclining to one side nearly at right angles from the stem, 1 to 3 inches long. Flower large, drooping, dark purple or white. Leaves 4 or 5

inches long and of the same breadth. Found during the fore-part of May in rich secluded woodlands.

Syn. T. rhomboideum. *Mich.*

65. Genus. MITELLA. *L.*

(*Deriv.* Latin, diminutive of *Mitra;* the ripe capsule resembling a cap, or little mitre.)

Class 10*th.* *DECANDRIA.—Order, DIGYNIA.*

Gen. ch. *Calyx* 5-cleft, persistent. *Petals* 5, pinnatifid, inserted into the calyx. *Capsule* 1-celled, 2-valved, valves equal.

Species 1*st.* Mitella diphylla. *Two-leaved Mitella.*

Sp. ch. *Leaves* somewhat lobed, with the lobes acute and dentate; *stem* erect with two opposite leaves about the middle.

Obs. This delicate little plant is readily distinguished by its cordate root-leaves on long petioles; its single stem, about one foot high, having, in the middle two opposite sessile leaves, and bearing at the top a spike of small white flowers, delicately fringed on the edges. It is rare in this locality, being only found occasionally on the moist and shaded borders of water-courses. Flowers early in May.

66. Genus. IRIS. *L.* *Flag.*

(*Deriv.* From the hues of the flower resembling in variety those of the *rainbow.*)

Class 3*d.* *TRIANDRIA.—Order, MONOGYNIA.*

Gen. ch. *Corolla* 6-parted, alternate segments reflected. *Stigmas* resembling petals.

Species 1*st.* Iris cristata. *Small crested Flag.*

Sp. ch. Bearded, *beard* crested; *scape* generally 1-flowered, as long as the leaves; *petals* nearly equal.

Obs. This tribe is readily recognized by the student from their near resemblance to the cultivated species of the gardens. The present is the only one native of this section, and is found on the rocky or gravelly margins of the larger water-courses. The flowering stems are generally about 6 inches high, bearing one or two handsome mottled flowers. The larger leaves are about the same length. The roots are fibrous and creeping, and when chewed impart a pungent sweetness to the taste. Kentucky river. Fore part of May.

67. Genus. VIBURNUM. *L.*

(*Deriv.* Probably from the Latin, *vieo*, to bind; from the use formerly made of some of the species.)

Class 5th. PENTANDRIA.—Order, TRIGYNIA.

Gen. ch. *Calyx* small, 5-parted, superior. *Corolla* campanulate, 5-cleft. *Berry* or drupe 1-seeded.

Species 1st. Viburnum prunifolium. *Black-haw.*

Sp. ch. *Leaves* obovate, nearly round and oval, glabrous, sharply serrate; *petioles* winged.

Obs. The Black-haw is a common shrub or small tree on the more broken and rocky lands bordering the Elk-horn and Kentucky river. Its small white flowers collected into dense heads are quite ornamental, in contrast with the deep glossy green of the leaves; and the fruit, which ripens in September, is sweet and palatable. Blooms about the 10th of May.

68. Genus. CELTIS. *L.*

(*Deriv.* An old name for an Asiatic tree; Etymology obscure.)

Class 5th. PENTANDRIA.—Order, DIGYNIA.

Gen. ch. Polygamous. *Calyx* 5-parted. *Corolla* none. *Styles* thick, divaricate. *Drupe* 1-seeded. *Masculine* flowers (inferior.) *Calyx* 6-parted, with 6 stamina.

Species 1st. Celtis occidentalis. *Hackberry.*

Sp. ch. *Leaves* ovate, acuminate, equally serrate, unequal at the base, scabrous above, pubescent beneath; *flowers* solitary.

Obs. The Hackberry is a very common tree throughout the best lands of this state, attaining in such situations a very great height. The wood splits well but is not durable, nor does it make good fuel. Its small inconspicuous flowers are unfolded during the first part of May, and are succeeded by berries about the size of small garden peas, the kernels of which are surrounded by a thin pulp, of very sweet taste, which has given to the tree, in some parts of the country, the name of sugar-berry-tree. These berries are eagerly sought after by birds, and the coarse harsh leaf of the tree is the favourite food of many insects.

In the western part of Kentucky a dwarfish species of this tree (*C. pumila,*) is met with, bearing fruit when not more than 2 feet high.

69. Genus. GYMNOCLADUS. *Lamark.*

(*Deriv.* Greek, *gymnos*, naked, and *klados* a branch; in allusion to the naked and seemingly dead appearance of the branches in winter.)

Class 22nd. DIOECIA.—Order, DECANDRIA.

Gen ch. Masc. *Calyx* tubular, 5-cleft. *Corolla* of 5 petals. Fem. Flower as the male. *Style* 1. *Legume* 1-celled, internally somewhat pulpy. *Seeds* roundish, large and indurated.

Species 1*st*. Gymnocladus Canadensis. *Coffee-nut-tree.*

Sp. ch. *Leaves* bipinnate; *leaflets* oval, acuminate, pubescent. *Pursh.*

Obs. The coffee nut, tree, as it is here universally called, is the only species of the genus, and is peculiar to the forests of Canada and the western states of North America, in no portion of which is it more commonly met with, or seen in greater perfection than in the immediate neighbourhood of Lexington. It is strikingly characterized by its naked trunk, covered with a rough contorted bark, lofty in proportion to its size; its thick obtusely pointed branches; its large compound leaves, formed of a great number of small leaffets, arranged by double pairs; and more particularly by its large brown pods, which remaining on long after the leaves have fallen, give a peculiar effect. The wood is porous and of a reddish hue, not unlike the coarse kind of mahogany: it splits well but is little used in mechanicks. The seed, of which 6 or 8 are found in each pod, are large and hard; the kernel, extracted by roasting in the fire, ground and boiled in water forms a drink not unlike coffee.

Although this tree is usually ranked among the dioecious vegetables, (in which the male and female flowers are borne on different plants,) I have been induced to believe that it was frequently polygamous or hermaphrodite, from observing that a barren tree is very rarely met with, and that individuals bear fruit abundantly although solitary and distant from all others of the kind. Pursh even places it in the class Decandria. Flowers generally from the 10th to 15th of May. Fruit ripens in October.

Syn. Guilandina dioica. *Linn.*

70. Genus. PTELEA. *L.*

(*Deriv.* Greek, *ptelea*, the elm; from the resemblance of the fruit of this shrub to that of the elm.)

Class 4th. TETRANDRIA.—Order, MONOGYNIA.

Gen. ch. *Corolla* 4-petalled. *Calyx* 4-parted, inferior. *Stigmas* 2. *Samara* compressed, nearly round, 1-seeded in the centre.

Species 1st. PTELEA TRIFOLIATA. *Shrubby trefoil.*

Sp. ch. *Leaves* ternate; *flowers* in panicles, polygamous.

Obs. A shrub of 8 or 10 feet in height with a smooth greyish bark. The leaves are trifoliate, or arranged in threes, of a bright glossy green on their upper surface but paler underneath. The flowers are not showy, but the seed being large and collected into clusters are quite ornamental. The whole plant has a strong unpleasant odour, not unlike that of the common buckeye (*Æsculus pallida*). Grows on stony lands bordering water courses. Rare in this neighbourhood. Flowers from 10th to 15th May.

71. GENUS. EUONYMUS. *L.*

(*Deriv.* So called by antiphrasis from *euonymos,* Greek, good name, because the plant was deemed infamous for its properties.)

Class 5th. PENTANDRIA.—Order, MONOGYNIA.

Gen. ch. *Calyx* 4 or 5-cleft, with a flat peltate disk at base, within; *Corolla* 4 or 5-petalled. *Capsule* 3 to 5-angled, 3 to 5-valved, coloured.

Species 1st. EUONYMUS ATROPURPUREUS. *Burning-bush. Spindletree. Indian arrow-wood. Wahoo.*

Sp. ch. *Leaves* petiolate, oblong-lanceolate; *peduncles* divaricate, *fruit* smooth. *Darl.*

Obs. The Indian arrow-wood or Wahoo, by which names this shrub is here universally known, is found in rich moist forests, not too much frequented by cattle. Its small flowers of a dark purple appear about the 10th of May, and are succeeded by numerous flesh-coloured, angular capsules, which bursting disclose its bright red seed attached by slender filaments: these continuing on the bushes throughout the fall, and even until mid-winter, give to the plant a highly ornamental appearance. The bark of the root is actively cathartic, and a decoction in water exhibited in small and frequently repeated doses, has proved highly beneficial in asthmatic cases.

Species 2nd. EUONYMUS AMERICANUS. *Small burning-bush.*

Sp. ch. *Branches* smooth quadrangular: *leaves* subsessile, smooth, acute, serrate: *peduncles* 3-flowered: *flowers* 5-cleft: *fruit* warty.

Obs. Much less abundant in this locality than the preceding, being confined to the rocky banks of water courses, and even there, not often met with: in the western part of the state it is frequently found in such situations. The flowers are less darkly coloured; the leaves smaller and persistent until late in the fall; the fruit exteriorly warty, but interiorly of the same bright colour. The whole plant is of humbler growth but equally ornamental. Flowers about the same time.

RANUNCULUS. (SEE GENUS 22.)

Species 5th. RANUNCULUS FLUVIATILIS. *River crow-foot.*

Sp. ch. *Stem* swimming; *leaves* all capillary, many-cleft; *petals* obovate, larger than the calyx; *seeds* glabrous.

Obs. A small aquatic plant found in shallow-running waters. Numerous stems arise in common from one spot and subsequently branching take root at each bifurcation; the leaves, which arise from the same points, are alternate, and finely dissected, giving the plant, before it blossoms, the appearance of a moss. The flowers are small, of a yellowish white colour, and arise about an inch above the surface of the water. Found in Elkhorn on the George-town road: rare: blooms about the 10th of May.

Syn. R. pantothrix. *Elliott.*

72. GENUS. STAPHYLEA. *L.*

(*Deriv.* Greek, *staphyle*, a bunch, or cluster; alluding to the manner of its fructification.)

Class 5th. PENTANDRIA.—Order, TRIGYNIA.

Gen. ch. *Calyx* inferior, 5-parted, coloured. *Corolla* 5-petalled. *Capsules* 2 or 3, inflated, connate. *Nuts* mostly 2, globose.

Species 1st. STAPHYLEA TRIFOLIA. *Bladder-nut.*

Sp. ch. *Leaves* trifoliate; *racemes* pendulous; *petals* ciliate below.

Obs. This shrub, attaining the stature of a small tree in favourable situations, is most generally found growing on the

rich declivities of hills and among rocks bordering water-courses. The pale, yellow flowers, produced abundantly in lax racemes, make a handsome show about the middle of May; and in the fall the shrub is conspicuous among all others by its large inflated capsules not unlike those of the ground cherry. (*Physalis.*) The wood is soft and valueless.

Syn. S. trifoliata. *Marshall.*

73. Genus. ORCHIS. *L.*

(*Deriv.* An ancient Greek name, alluding to the testicular shape so common in the roots of this genus.)

Class 18*th.* *GYNANDRIA—Order, MONANDRIA.*

Gen. ch. *Corolla* ringent, upper leaflet vaulted. *Lip* dilated, base spurred beneath. *Pollinia* 2, terminal, adnate.

Species 1*st.* Orchis spectabilis. *Showy Orchis.*

Sp. ch. *Lip* obovate, crenate; *spur* clavate, shorter than the germ; *bractes* large.

Obs. Of this extensive genus, the present subject is the only species I have met with in this neighbourhood, where, indeed, gynandrous plants of any kind are rare. A few years since this occurred frequently in moist rich woods; it has now, however, almost disappeared before cultivation and the ravages of cattle. When met with it is readily recognized by its particoloured flowers, the upper lip being purple, the under one and spur a pure white; by its two radical leaves sheathing the stem below; by its angular scape, and large bractes. Flowers about the middle of May.

In other parts of the state the *Orchis ciliaris* and *O. fimbriata* are abundant.

74. Genus. BIGNONIA. *L.*

(*Deriv.* Named by Tournefort in honour of the *Abbe Bignon*, a learned Frenchman of the 17th century.)

Class 14*th.* *DIDYNAMIA.—Order, ANGIOSPERMIA.*

Gen. ch. *Calyx* 5-toothed, cup-shaped, coriaceous. *Corolla* campanulate, 5-lobed, ventricose on the under surface. *Pod* 2-celled. *Seeds* winged with a membrane.

Species 1*st.* Bignonia capriolata. *Yellow creeper. Cross vine.*

Sp. ch. *Leaves* conjugate, bearing tendrils on the petioles, leaflets ovate, acuminate, sub-cordate, at base; *racemes* axillary.

Obs. This handsome climber occurs frequently among the mural cliffs of the Kentucky river, sometimes covering the face of the rock to a considerable extent; attaching itself by tendrils which arise from the foot-stalks of the leaves. The leaves are in pairs, two arising in common, and opposite to two others; they retain their verdure partially throughout the winter. The flowers are numerously produced at the axils of the leaves; they are of a reddish colour exteriorly and yellow within, appearing about the middle of May, but so transient in duration as not often to be met with in bloom.

Syn. B. crucigera. *Walter.*

Species 2nd. BIGNONIA RADICANS. *Trumpet flower.*

Sp. ch. Leaves pinnate, leaflets ovate, dentate, acuminate; *corymb* terminal; *tube* of the *corolla* twice as long as the calyx; *stem* radicant.

Obs. No vine is more common in the forests of this country, or more commonly planted for ornament, than the trumpet flower; climbing over buildings and the loftiest trees, throwing out radicles all along the stem by which it attaches itself firmly to walls, fences, or the bark of trees. Its large red flowers, produced in clusters, are highly ornamental; they appear somewhat later than those of the preceding species, and are succeeded by long pods filled with membranaceous seed. Some circumstances have occurred within my knowledge which induce me to think this plant is always injurious and sometimes fatal to the tree supporting it.

75. GENUS. RHUS. *L.*

(*Deriv.* Probably ftom the Celtic *rhudd,* red; in reference to the colour of the fruit.)

Class 5th. PENTANDRIA.—Order. TRIGYNIA.

Gen. ch. Calyx 5-parted. *Petals* 5. *Berry* small, sub-globose, with one bony seed.

Species 1st. RHUS RADICANS. *Poison oak. Poison vine.*

Sp. ch. Leaves ternate, leaflets petiolate, ovate, glabrous, generally entire; *stem* radicant; *flowers* dioicous.

Obs. This vine is generally known from all others by its poisonous properties, producing swelling, inflammation and a peculiar eruption on the skin of most persons touching it; some constitutions, however, are much more liable to its deleterious impression than others. It is extremely common in this section of

the Union, covering almost every dead tree, and climbing to the tops of the loftiest branches, attaching itself by radicles from the stem in the same manner with Bignonia radicans. Its clusters of small greenish coloured flowers, appear towards the middle of May, and are succeeded by dark berries not unlike the small winter-grape.

Syn. R. toxicodendron, (*Michaux*,) which is regarded by some as a mere dwarfish variety of the present plant.

Species 2nd. RHUS GLABRUM. *Common Sumach.*

Sp. ch. *Leaves* pinnate, leaflets lanceolate, acuminate, acutely serrate, villous underneath.

Obs. This, the only other species of Rhus found in this neighbourhood, is universally known by the name of Sumach. It is frequently introduced into shrubberies and forms a pleasing object in ornamental gardening; its rich drooping foliage having somewhat a tropical appearance. The flowers appear considerably later than those of the preceding species, and are produced in dense conical clusters, succeeded by clumps of red berries, which are covered with a mealy efflorescence of very acid properties. In autumn the leaves assume a scarlet colour which adds much to the beauty of its appearance, and the berries hang on during the winter. The berries and leaves are used in tanning morocco.

The *Rhus copalinum* is common in the *barrens* of Kentucky.

76. GENUS. HYDRANGIA. *L.*

(*Deriv.* Greek, *hydor*, water, and *aggos*, a vessel; in allusion to the pitcher-like shape of the seed-vessels.)

Class 10th. DECANDRIA.—Order. DIGYNIA.

Gen. ch. *Calyx* superior, 5-toothed. *Petals* 5. *Capsule* 2-beaked, opening by a foramen between the horns.

Species 1st. HYDRANGIA CORDATA.—*Pursh.*

Sp. ch. *Leaves* broad ovate, slightly cordate at base, acuminate, coarsely toothed, glabrous underneath; *cymes* generally radiate.

Obs. This Hydrangia occurs frequently on rocky situations near water throughout the Western country. The leaves are large, opposite and heart-shaped. The flowers produced in cymous clusters are composed of small central fertile florets, whilst those of the circumference are larger, more showy and

barren. On the Kentucky river: growing to the height of 4 or 6 feet: flowers middle of May.

Syn. H. vulgaris.—*Mich.*

77. Genus. CYNOGLOSSUM. *L.*

(*Deriv.* Greek; *kyon, a dog*, and *glossa*, a tongue, from a fancied resemblance in its leaves.)

Class 5th. PENTANDRIA.—Order, MONOGYNIA.

Gen. ch. *Calyx* 5-parted. *Corolla* short, funnel-formed, the the orifice closed with connivent scales. *Seeds* depressed, fixed to a central column.

Species 1st. Cynoglossum officinale. *Common hounds-tongue.*

Sp. ch. *Leaves* broad-lanceolate, attenuate at the base, sessile, downy; *stamens* shorter than the corolla.

Obs. This plant, originally exotic, has been gradually dispersed over the greater part of the Union. The whole plant is of a dull green and downy; lower leaves broad and lanceolate, not unaptly giving name to the genus from their similitude in form to a dog's tongue: upper leaves narrower, more pointed and sessile. The stems arise to the height of about 2 feet, branching towards the top; each branch bearing a spike of small, dark purple flowers, which are succeeded by large seed covered with prickles, by which they adhere to fleeces or clothes. Common in pastures and waste grounds. Flowers about middle of May.

Species 2nd. Cynoglossum virginicum. *Virginian hounds-tongue.*

Sp. ch. *Leaves* oval-oblong, acute, hairy, upper ones cordate and amplexicaul at base; *corymb* on a long naked peduncle.

Obs. Differs from the preceding in the colour of the flowers which is a bluish white, it is moreover less branched and more hirsute. Found in oak woods, though less abundantly than the former species. Flowers about the same time.

Syn. C. amplexicaule. *Mich.*

78. Genus. VERBASCUM. *L.*

(*Deriv.* A corruption of *Barbascum*, from *barba* a beard, on account of the hairy or woolly leaves of some of the species.)

Class 5th. PENTANDRIA.—Order, MONOGYNIA.

Gen. ch. *Calyx* 5-parted. *Corolla* rotate, 5-lobed, unequal.

Stamina declined, bearded. *Stigma* simple. *Capsule* 2-celled, valves inflected, many seeded.

Species 1*st.* VERBASCUM THAPSUS. *Common Mullein.*

Sp. ch. *Leaves* decurrent, woolly on both sides; *raceme* spiked, dense, two of the *stamens* glabrous.

Obs. Every one is familiar with this very common plant, which though originally introduced is now but too thoroughly naturalized among us. Popular opinion has long pronounced it possessed of considerable medical virtue; and Dr. Smith, of London, says "the whole herb is mucilaginous, emollient, and sometimes narcotic." Abundant in old fields, stony grounds, and washed hill-sides. Begins to bloom towards the middle of May and continues flowering until fall.

Species 2*nd.* VERBASCUM BLATTARIA. *Moth mullein.*

Sp. ch. *Leaves* amplexicaul, oblong, smooth, serrate; *peduncles* 1-flowered, solitary.

Obs. A less luxuriant plant than the former, and although a foreigner also, equally abundant. It is found in similar situations with the common mullein, and is frequently seen growing between the bricks of pavements and moist walls. The stem is ordinarily about 2 feet high, densely clothed at the bottom by smooth serrate leaves, and bearing on the summit a spike of white flowers tinged internally with purple; the anthers being a bright orange colour. These appear ordinarily towards the 15th of May, continue evolving for some weeks and are succeeded by dry, roundish capsules filled with small seed. The variety of this species with yellow flowers I have not seen in this country.

79. GENUS. OXALIS. *L.*

(*Deriv.* Greek, *oxys*, acid: in reference to the quality of the plants.)

Class 10*th.* *DECANDRIA,—Order, PENTAGYNIA.*

Gen. ch. *Calyx* 5-leaved. Petals 5, connected by claws. *Stamina* alternately shorter. *Capsule* 5-celled, 5-angled.

Species 1*st.* OXALIS STRICTA. *Sheep-sorrel. Wood-sorrel.*

Sp. ch. Caulescent; *stem* erect, branched, hairy; *leaves* ternate, obcordate; *umbels* longer than the petioles; *petals* obovate, entire; *styles* as long as the interior stamens.

Obs. Although considerable diversity obtains, according to

the season, in the time of the sorrels' flowering, yet the middle of this month may be regarded as the medium period: however it is occasionally found in bloom much earlier. This species is distinguished by its upright hairy stem, branching towards the top, and bearing several umbels of small yellow flowers. It is common in gardens and other cultivated grounds.

Species 2nd. OXALIS VIOLACEA. *Violet wood-sorrel.*

Sp. ch. Stemless; *scape* umbelliferous, *flowers* nodding: *leaves* ternate, obcordate, glabrous; *styles* shorter than the stamens, recurved.

Obs. Easily distinguished from the preceding by its lower growth, for having no proper stem the leaf and flower-stalks arise from the root, which is tuberous. The leaves are moreover larger than those of *O. stricta*, and are marked with purplish stains: the flowers are also larger and of a handsome violet hue. Grows in shaded woods or damp soils, blooming first about the middle of May; Mr. Nuttall remarks that it appears often to flower again late in the Autumn, and is then destitute of leaves.

80. GENUS. ANTHEMIS. *L.*

(*Deriv.* Greek, *anthemon*, a flower; from the profusion it bears.)

Class 19*th*. *SYNGENESIA.—Order, SUPERFLUA.*

Gen. ch. *Calyx* hemispherical, scales nearly equal. *Corolla*, rays more than 5. *Pappus* none or marginal.

Species 1*st*. ANTHEMIS COTULA. *May-weed. Dog-fennell.*

Sp. ch. *Receptacle* conic, chaff setaceous; *seed* naked; leaves bipinnate, leaflets subulate, 3-parted.

Obs. The road sides and waste grounds are every where covered with this obtrusive weed, which, originally introduced, has become quite too well known among us. It commences flowering about the middle of May and continues from that time till the coming of frost, to put forth blossoms, and ripen its seed. The whole plant has strong sensible properties and is reputed medicinal; it is however entirely destitute of the aroma of the cultivated chamomile, *A. nobilis*.

[TO BE CONTINUED.]

NOTICES.

We have received in addition to the usual exchanges, since the publication of the last number,

1. Medical facts and inquiries respecting the causes, nature, prevention and cure of Fever: more especially in relation to the endemic fevers of summer and autumn in the Southern States. Together with a history of the bilious remitting fever of Alabama, as it appeared in Cahawba and its vicinity in the summers and autumns of 1821 and 1822. By Jabez W. Heustis M. D. (From the Author.)

2. An Essay on the non-existence of Malaria; especially as a cause of Intermittent and Remittent Bilious fevers. Read before the Central Medical Society of Georgia, December 3d 1828 by Alexander Jones, M. D (From the Author.)

A copy of the same from Dr. Allen Kimbal.

3. New views of Penitentiary Discipline, and moral Education and Reform. By Charles Caldwell, M. D. &c. (From the Author.)

Several communications have come to hand, too late for insertion in the present number.—They shall be attended to hereafter. Those intended for the November number should be forwarded as early as possible.

We regret to state that owing to the loss of certain apparatus at the late fire, Professor Matthews has been prevented, for the present, from continuing his useful meteorological tables.

The New-York Prize Medal, value one hundred dollars, offered for the best essay on Typhus Fever, has been unanimously awarded by the Committee, to Alfred Thruston Magill M. D. of Winchester in Virginia.

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MR. NICHOLSON'S CASE.

Mr. Nicholson, of Baltimore, having issued a second publication against Dr. Dudley of Lexington, Kentucky, which has arrived in this place when the Doctor was making arrangements to travel to the North, a friend has determined to make a few remarks on the whole business, which he hopes will tend to place it in a proper light.

Dr. Dudley in an Essay on the use of the Bandage in Fractures of the Bones, stated a case which Mr. Nicholson asserts is his: but Dr. Dudley mentions no name. Had Mr. Nicholson said nothing about it, not one in ten thousand could have known who was the subject of the accident.

Mr. Nicholson has taken great offence at the statement of his case, and has repeatedly mentioned it in the course of his two papers, in terms of the strongest indignation. But in this Dr. Dudley has only done what all practitioners, who write, continually do, both in Europe and America; and what the interest of humanity requires they should do. Indeed, objection to this practice is unheard of, so far as the writer's information extends. On the contrary, it is a very common thing to state the cases of individuals, even of high rank, with their names annexed. If practitioners are not at liberty to state cases elucidating their views, and showing the propriety of their practice, the best of medical writings will be at an end, and the science must cease to progress; and if cases are not to be stated in lectures to students, Universities may be closed.

So much for the propriety of the publication *in general* of such cases.

As to this particular case, suppose for a moment that it had not been mentioned. Those who know as much of this matter as the writer does, will be inclined to believe that *the omission* would then have been the ground of attack. It would then have probably been asserted that Dr. Dudley was conscious of mismanagement in this case, and dare not say any thing about it.

Mr. Nicholson has not only objected to the statement of his case, although without mention of his name, but has called in question the judgment of Dr. Dudley, with regard to the nature of the injury, and his skill in treating it; and particularly reprobates the bandage and attributes to it the loss of his leg. A few remarks on these points.

Dr. Dudley's skill in Surgery has placed him *alone in the West.* We have heard of no man who pretends to be his rival. His aid is continually sought by numbers from every Western state and Territory; and not a few from the East, after having sought relief without finding it from the most celebrated Surgeons of the Atlantic states, have been indebted to him for unhoped restoration to health. The pressure of his

business has long been such that he has been careless of it, and totally devoid of anxiety respecting it, except as to the load of care attending it. In these circumstances who can suppose that, when called upon to state the nature of Mr. Nicholson's injury, he would be incapable of deciding whether a bone, so completely exposed to examination as the head of the fibula was or was not fractured? We aver that it is next to impossible for a student to mistake such a case. But it is said that many medical men who have since examined the bone, declare it was not fractured. We aver that they have been guilty of presumption who have said so. For, on the one hand, as able and experienced a surgeon as America can boast of declares that it was broken, when he had every opportunity of deciding fairly, and when he had no conceivable interest in mis-stating the case. On the other, those who have said (and most of them too with but slender pretensions to surgical knowledge) that the bone was not broken, decided upon the external appearance of the bone above three years after it was healed; when they themselves (as is presumed from Mr. Nicholson's statement on the subject, which he must have derived from some of them,) are aware that it is strongly contended by European surgeons, that it is impossible to discover the difference (even by an examination of the interior of the bone,) between a bone that had been fractured and healed, and one that had not; and those who think otherwise have so completely failed to show any difference, that the question is according to Mr. Nicholson, yet unsettled.

It is evident therefore that those, who knew the decision of Dr. Dudley at the moment of the accident and who were sufficiently acquainted with the subject to know that the question, *of the possibility of determining by the examination of a bone whether it has been broken or not*, is unsettled; and consequently, that *the certain determination* of the question in any given case is impossible;—it is evident that they acted presumptuously. As to those of the signers who are acquainted with the difficulty of such a decision, but did not know that, with the best opportunity of ascertaining the state, an experienced and able Surgeon had decided that the bone was broken, even they cannot escape the charge of venturing a decision without any grounds on which to form an opinion with certainty.

Certainty then is absolutely out of the question on the side of those who say the bone was not broken. Uncertainty is as completely out of the question when a man can put his finger on the bone and feel the grating of the pieces.

As to the charge of the injury having arisen from the application of the bandage, we have only to say that it is impossible; because this practice has been followed in thousands of cases in the West, and every day's experience shows its immense advantages. Thousands have experienced it; thousands are witnessing it every year; thousands are practising it. The writer has himself seen it used in a variety of cases, and never with bad effect. If the bandage had been the cause of sloughing in Mr. Nicholson's case, it must have produced similar effects in other similar cases. That it has not, the writer is well persuaded of.

One of his servants had, within a year, his leg broken and Dr. Dudley cured it by bandaging alone. He is now one of the best in the country.

The effects of a bandage in producing sloughing must appear very speedily. Mr. Nicholson states himself, in his first publication, that the sloughing did not occur until almost a month after the accident.

A bandage put on irregularly, so as to prevent the circulation of the blood through the limb, would no doubt end in mortification. But in that case the patient could not live a week; and in the case in discussion not half the time. Here then the sloughing was not produced by the bandage as put on by Dr. Dudley. The sufferings of the patient were then produced by some other cause. What that cause was, cannot be questioned by those who know the parties in Lexington who say, some of them unwillingly, (having incautiously mentioned the facts before the matter was agitated, and now being unable to draw back) that Mr. Nicholson did act with extreme imprudence in diet, &c. It is not necessary to say more on this subject than that the truth of the statement is supported by many: by some who have voluntarily come forward in support of Dr. Dudley; by some who have done it unwillingly, as above mentioned; and by some who have not been publickly mentioned as yet. The subjoined letters to Dr. Dudley were voluntarily forwarded to him. The circumstances which produced the two former were of course unknown to him; viz. the application of Mr Nicholson to two of the witnesses.

C.

Baltimore, 20th March, 1829.

DEAR SIR,

I have been induced to write to you, inasmuch as Dr. Dudley of Lexington in his late most extraordinary publication, has introduced your name among others, and refers me to you as one of the sources of his information, as respects my irregular conduct while confined at the house of a friend in Lexington; and while under his care; and again refers the public to you for confirmation of what he has stated.

Having recently received a letter from Lexington stating that the Dr. did not receive his information from *the family*, induces me to believe that he has also gratuitously introduced your name, to make plausible his unfounded story. Before I notice his production I wish you to inform me, whether his proceedings, as respects yourself, were by you sanctioned, or if as he states you gave him the information which he refers to: Presuming he has sent you a copy of his publication, I shall only give you a statement of his leading charges. In his first essay he says "notwithstanding a variety of imprudences committed by the patient, in rolling in bed; in *cating and drinking secretly* articles of the *most injurious kinds* &c. &c. Again in the 18 and 19 pages of his late production, he says "Besides various articles of drink and diet &c. consisting of Brandy, Wine, Porter, Cider, Oysters, cheese and crackers,

my plate startled those who observed it, and the cravings of my appetite *were* such that Mr. Beatty informed him I was in the habit of *ordering* a dish to be set aside to satisfy my hunger &c. "for his (my) satisfaction I will refer him to Dr. Stephens R. Beatty of Washington County Maryland, and to Mr. Charlton Beatty of Kentucky. Your early answer will oblige Your humble servant,

WM. P. NICHOLSON.

Dr. Stephens R. Beatty, Washington Co. Md.

Winchester, Frederick County, Va.
29th March, 1829.

DEAR SIR.

Your letter of the 20th instant was duly received at Clear-Spring, my place of residence heretofore, but as I had not then seen Profesor Dudley's reply to your pamphlet, and expected to find it the office on my arrival here, I have deferred answering your letter until now.

I have examined the Doctor's reply and find that he has correctly represented the information he had received from me.—I spent the winter of 1827-8 in Lexington, and visited occasionally Mr. Dewees' family, in which a cousin of mine, Mr. Charlton Beatty, resided. Your case became the subject of general conversation one eveing at the tea-table, and from the conversation my impression was, that while under the care of Doctor Dudley you had made use of articles of diet which were contrary to his directions.

I have hastened to answer your letter immediately after my arrival here, and hope the above apology will be sufficient for having delayed it a few days.

Yours Very Respectfully,

STEPHENS R. BEATTY.

MR. WM. P. NICHOLSON.

Baltimore 20th of March, 1829.

DEAR SIR,

I am induced by the late extraordinary publication of Dr. Dudley to address you, inasmuch as he refers me to you among others for the sources of his information as respects my irregular conduct while confined at the house of our mutual and highly esteemed friends in Lexington. I am led to believe by the variety of articles enumerated in his published bill of fare (independent of the *back bone of a hog* which he says you told him I was in the habit of ordering to be set aside for my benefit between meals) that it has been somewhat of a gratuitous piece of information and business on the part of Dr. Dudley, to make plausible his unfounded story. Before I undertake to answer him I wish you to inform me whether his proceedings as respects yourself, were by you sanctioned, or if as he states, you gave him the information referred to in the 18th page, and in the 19th page, where he refers again to you for

confirmation of what he has published. Your early answer will much oblige

Your obedient servant,

WM. P. NICHOLSON.

Mr. Charlton Beatty, Lexington, Ky.

Maysville, April 5th, 1829.

DEAR SIR,

My having left Lexington, to which place yours of the 20th March was directed, will I hope by you be deemed an ample apology for my delay in replying to it. As I have no copy of the pamphlet to which you refer, I am compelled to answer entirely from memory. I told Dr. Dudley that large portions of animal food were daily sent from the table to your room; that in addition to the articles of drink admitted by yourself, a glass of brandy toddy was on one occasion at least prepared for you. I told him also that I had heard an order to set up the cold chine for your benefit between meals. I do not recollect all the conversations I may have had with the Doctor on the subject, but I believe I never gave a more specific *bill* of *fare* than the above. He was authorized to use my name.

I remain your Obedient Servant

J. C. BEATTY.

Alexandria, La. May 4th, 1829.

HONOURED SIR,

During my visit to Cincinnati I met with Mr. Nicholson's pamphlet, and your reply. The contents of that Pamphlet was a matter of much surprise, and I cannot find language sufficiently strong to reprobate the false and illiberal statements that it contains. A vain and futile attempt has been made to pervert the case, and to represent it to the world under a new and false garb. But truth is mighty and, must prevail.

I recollect distinctly, that late in the evening you were sent for in great haste to visit Mr. Nicholson at the house of Mr. Dewees. On our arrival, we found Mr. Nicholson extremely restless and complaining of excessive pain. He gave you the following history of his own case. He observed that in mounting his horse, and before he had properly gained his saddle, that his horse leaped off suddenly, threw him with his leg on a piece of timber which was in the yard, and had fractured his leg. The patient was immediately placed on the floor, and the limb examined. On an examination it was found that the head of the Fibula was fractured, and gave to the hand the sensation of being crushed into a number of fragments.

The crepitation and motion of the fragments of bone was perfectly audible and distinct, and every symptom of a fractured bone was so

manifest and unequivocal as not to leave the shadow of a doubt in relation to the existence of a fracture. Mr. Nicholson was told by you that there was a fracture of the head of the Fibula, and not a sprain. The limb was dressed in your usual manner of dressing fractures, with the Bandage. The bandage was passed from the toes to some distance above the knee and made fast. The bandage was applied smooth and regularly, so as to give an equable and uniform pressure to the whole limb, and not with "a ten horse power." The extreme pain and restlessness of the patient before dressing of him, was supposed to be produced from the injury which the nerves had sustained. The patient being dressed, he was placed upon a mattrass, and ordered to be kept perfectly quiet. But in violation to all orders, and contrary to the dictates of common sense and prudence, he tossed and rolled over the bed during the night. On the following morning the limb was properly adjusted; and rest and quietude enjoined strictly on the patient and nurses.

On the second day in the morning I visited the patient by your request, I found him in great pain, and immediately I proceeded to ascertain the cause, and to my great astonishment Mrs. Nicholson observed to me, "Sir you know the curiosity of our sex, I wished last night to know how the limb looked and I took my scissors and cut the bandages loose about the ancle." The bandages were loose about the instep and foot, and perfecly tight on the leg and knee. Tumefaction, swelling and pain of course were the consequences. The bandages were removed and properly applied again. The patient done well for several days until great excesses were committed in diet and drink. Oysters and wine were indulged in to a considerable extent and two attacks of fever excited from them. The dressings were also repeatedly interrupted, and the patient was altogether the most restless and ungovernable that I ever witnessed. Notwithstanding, it is a matter of fact that in 22 or 23 days the patient was well enough to go to the country; such was the efficacy of the bandage. The frequent interruption of the bandage, the excesses in diet and drink, his great restlessness, and his violation to orders in general, are facts notoriously known to every member of the house. I not only saw decanters of wine setting in his room, but was told from the proprietor of the house and by Mrs. Nicholson that it was impossible to prevent him from committing imprudencies. I might state further on the case but have no room in this letter.

Female character is sacred and delicate, but truth and justice have demanded from me what I have said in relation to the subject, and I feel no hestation in stating what I have said. I should be happy to hear from you frequently.

Your friend and pupil

R. R. CUNEY.

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ORIGINAL COMMUNICATIONS.

ART. I.—*Further thoughts on Climate, as the remote predisposing cause of Autumnal Diseases.* *By* RANSOM MALONE COLLINS M. D., of East Feliciana, Louisiana.

(CONTINUED FROM p. 501, VOL. 1. NO. 4.)

"CLIMATE (speaking medically) may be defined to be that peculiar constitution of the atmospheric fluid, which immediately invests the earth, and in which all animals move and breathe." In every region of the earth there are some peculiarities, in relation to this fluid, which are cognizable to our senses; others of which chemical analysis alone gives us a knowledge; and others, I doubt not, of whose existence the imperfect method of conducting our chemical experiments gives us no intelligence. But with these last, the philosopher has nothing to do; having no evidence of their existence, he has no basis on which to found belief, and they are consequently to him a nonentity. Belief without evidence is the height of credulity, and never applies to the true philosopher. That we may imagine a thousand things, in relation to the causes productive of our moral or physical evil is certainly true; but we should be able, (in giving them to the world) in order to produce conviction, to

trace a rational dependence of effect upon cause, throughout the whole chain of causes and effects. An assumption is simply "ego dico," and cannot be expected to be taken as fact, unless by a philosophical process of reasoning, both analytically, and synthetically, we are convinced of the truth of such assumption.

Those who contend so strenuously for the existence of marsh miasmata, as the remote cause of fevers, and more especially those who assert that this substance has the effect of darkening the blood, have never thought proper, it seems, to inform us by what process this phenomenon takes place. Is this the direct effect of miasmata? Can this reach the circulation except by inflicting disease on the solids? Through what medium does it arrive thither, whether by the lungs, skin, or stomach? Does ague ever occur, but as the result of a weakened action of the heart? Does common phlegmonous inflammation have this effect on the heart? Is there any analogy existing between ague produced by the suppurating process in a common phlegmon and that which ushers in fever?

The fact that blood drawn from persons labouring under fever, or from those in whom we may fairly infer the existence of a strong predisposition to fever, is dark-coloured, is by no means conclusive, nor even strong presumptive evidence of the existence of miasmata. This can only go to show that some cause is operative in producing this effect, and if this condition of the blood be absolutely necessary to the existence, or at least to the production of our autumnal fevers, (which I greatly doubt) we should be authorized to attribute it to that cause which by lessening the amount of watery particles in that fluid, would produce a greater concentration of the colouring matter; or to a want of secreting from it that peculiar substance to whose presence it owes this dark colour, which is observed as well in healthy individuals as in the sick, though perhaps not in so great a degree. There can be no difference in dark blood, whether we assume that this is the effect of the action of miasmata, or that it is simply that kind of blood termed venous. Venous blood is incapable of stimulating the left side of the heart, with as much certainty as blood supposed to be darkened by the action

of miasmata. But that the debilitated action of the heart depends at all, upon the presence of dark blood is wholly gratuitous and cannot be proven. We might with the same plausibility contend that, that peculiar listlessness, and sense of muscular relaxation, with a disinclination to active exertion, want of appetite, imperfect digestion, constipated condition of bowels which usually, and most of them, always accompany the access of fever, depended on the presence of dark blood. The whole system is debilitated, the heart no more than all the other muscles.

But, to return from this digression to the consideration first proposed, I say, in every region there are certain peculiarities of climate cognizable to our senses; and in accordance with these, is the term climate used by physicians. We consequently have every variety of climate, which can be imagined to be made up of all the vicissitudes of heat and cold, moisture and aridity. But amid all this variety, there appears to be but one agent whose presence or absence gives specific character to every district of country. This is caloric. We consequently have the warm dry, the warm humid, the cold dry, and cold humid climates; and these perhaps all within a few miles, owing to local circumstances. It is this substance which constitutes every variety of climatorial impression, from that which freezes within the polar regions of the north, to that which scorches us on the burning plains of the south; from the healthful breeze which fans us in middle latitudes, to the burning sirocco whose blast is death. That this, as before observed, should exert a powerful influence on man, whether we view him in his moral or physical character, is not at all extraordinary. But man alone is not the subject of its control. All animated nature feels the modifying influence of this powerful agent. The humble plant owns and manifests the influence which heat exerts over it, not only by changes in its physical, but in its medical attributes. Certain plants by a removal from one climate to another, either lose their medicinal properties or have new ones superadded: nor is this change to be attributed to soil alone. It is a well known fact, that in the south the poppy yields the best opium, whilst that raised in col-

der climates possesses its narcotic principle in a very imperfect degree. Cruciform plants are said by Paris, to degenerate within the tropics, whilst they acquire increased energies in colder regions.* It is to the presence of caloric that in a very considerable degree we owe the beauty and highly finished colouring of the flowers which adorn our gardens and parterres; cold being capable of altogether suppressing the colour of the flowers and leaves of plants, as witnessed in the amaranthus and ranunculus of Lapland and Siberia.† But the effect of caloric as exhibited in different regions of the earth, is not only to be seen in the various mutations of structure and medical virtue of plants, but is most strikingly manifested in the lower order of animals, not only in their structure but in their propensities. Who has not been struck with the difference between the African and South American Tiger? The one implacable, cruel, and sanguinary in the extreme, defying all the attentions of its keeper; while the other seems capable of being excited into gratitude for favours bestowed. The one with a heart extremely capacious and powerful, would seem from the force and frequency with which the blood is thrown upon the brain, to be labouring constantly under partial phrenitis; whilst the other with a less powerful circulatory system, would seem to manifest marks of kindness and forbearance. The one appears to partake in his disposition of that ardour which characterizes his climate, scorched by a tropical sun, the intensity of which appears to be delineated in that blood thirsty disposition which he never fails to manifest. The other, blessed not only with plenty but a temperate climate, gives strong proofs of his better condition, by a corresponding mildness of disposition.

But the lower order of animals, under the influence of climate, are not less different in their structure and disposition than man. In whatever latitude man locates himself, he assumes certain characteristic modifications, purely the result of the influence of that particular atmosphere, and the various objects which surround him, with which he is about to establish a relationship. These modifications are so easily discernible, that we are

* Paris's Phamacologia, p. 82. † Ibid. p. 82.

daily in the habit of assigning the residence of the individuals with whom we chance to meet, to certain districts of country; especially where those districts of country are considerably removed from each other; for man bears the attributes of his climate, indelibly impressed upon him, in characters so plain that all may read. The sallow and swarthy individual whose complexion gives unequivocal evidence that all is not well within the portal circle, but that disease has riveted itself upon the spleen, liver, or some one of the abdominal organs, is immediately recognized as an inhabitant of the warm regions of the south, where autumnal diseases rage with most violence: while those whose aspect more especially combines the attributes which are ascribed to the sanguine temperament, are known to be the inhabitants of the mountainous districts of the north; and those whose features are bloated and pale, and aspect leucophlegmatic, are assigned with justice, to cold damp districts of country, or valleys surrounded by hills; such as the valley of Cashmere, or those found in Switzerland, and various parts of Europe and America. With no less facility does the close observer decipher the physiognomy of climate, in all its gradations, from heat to cold, from dryness to humidity. But this diversity of external appearances, the effect of climate, is not the entire distinguishing characteristic between man and his fellow man, the inhabitants of different regions. There obtains a striking difference, not only in their physical, but their moral efficiencies and pursuits; and man would seem to be what he is, more from necessity than is generally admitted. But with his physical condition, is the physician more especially concerned, and, as before stated, this difference is manifested, not only in his external appearances, but in his internal conformation. By this I would not be understood to mean, that there is an addition to or subtraction from the whole amount of organs of which he is compounded; but anatomy abundantly shows that there does take place certain changes which may and do amount to an increase or diminution, not only of the size, but of the powers of certain organs; and that in warm regions the abdominal organs are those which undergo certain changes, or suffer a loss of resis-

tive capacity, by which they are rendered more obnoxious to disease: whilst the thoracic are almost entirely exempted from primary attacks, and become diseased only in consequence of associated action. The very reverse of this takes place in cold countries. Anatomy, as just remarked, shows us that the liver and spleen of individuals who live and die in southern latitudes, are not only larger, but are much more flaccid and loose in their texture, than the same organs, in individuals who have lived in colder regions; whilst most probably some like difference obtains in the thoracic viscera, thus constituting what might not be improperly called an abdominal and a thoracic temperament. That these are purely the result of climate, would appear to me undeniable; for common observation has long since established the fact, that in proportion to the coldness of the climate will thoracic diseases prevail, and in proportion to its warmth will abdominal preponderate. To what then shall we look as the agent which so modifies and directs diseased actions? The answer is obvious; namely, caloric. The presence or absence of this powerful agent may be truly said to constitute a new set of temperaments, or predispositions, which mark the character of diseases.

We are further acquainted with the fact, that on the application of certain agents to the system, in cold countries, there will be a determination of the fluids, more especially to the lungs, and thoracic viscera; whilst the application of the same agent to the system of those living in warm countries, causes a determination to the liver, spleen, and abdominal viscera. Does congestion ever take place in an organ unless invited thither, by the peculiar condition of that organ? What condition of an organ is most favorable to this state? I am myself of opinion that congestion, or indeed a powerful determination to an organ, never takes place, except as the result of a debilitated condition of that organ, produced by some cause whereby it is rendered incapable of sustaining itself under an accumulation of fluid, which is thrown upon it by those organs which are in a more vigorous condition.

It would hence follow that the remote predisposing cause,

produces in certain organs a debilitated condition, which predisposes them to disease, and which gives character to disease. And I can readily conceive that, whatever organ or organs are designed to bear the "onus morbi" from the peculiarity of the remote predisposing cause, that organ or set of organs will thereby acquire an aptitude for disease, and thither will it be invited, every attack rendering the determination the more imperative. If I am correct, then, in my views on this subject it would follow that the excessive heat of our summers, by its debilitating influence on the system, proves the remote predisposing cause of our bilious remittents, intermittents, cholera, and dysentery, with all their train of morbid effects; whilst its absence proves the predisposing cause to thoracic diseases, as exemplified in phthisis, pneumonia, pleurisy, together with those diseases, which are termed inflammatory. It is evident, that whatever agent is operative in producing in an organ an aptitude for disease, is the remote predisposing cause of that disease, and if a debilitated condition of an organ, is congenial to the production of disease, whatever agent is capable of producing this condition proves the cause.

It requires no ingenuity to prove that long exposure to heat, is capable of producing debility, not only in an individual organ, but the entire muscular system becomes relaxed, and those organs more especially become so, which are, by the influence of this agent, thrown into a preternatural state of excitement, by which their excitability is exhausted, and they left in a torpid and inactive condition, which condition, is equally well known to be highly favourable to the induction of disease. In cold climates the system already wound up to the highest pitch by its invigorating influence, is always ready when a disturbance (by any cause) of the healthy balance of the various organs takes place, to take on inflammatory action, and those organs which are most delicate in their structure are the first to suffer lesion. Observation has shown that in proportion to the increase or diminution of the contaneous perspiration, so will be the determination to the lungs and thoracic viscera, constituting there a centre of fluxion, in all cold regions; consequently ren-

dering them more obnoxious to disease; and might I not be sanctioned in saying that a climatorial debility is induced in them, which renders this determination unavoidable.

I am well aware that occupation and mode of living have a powerful influence in forming our predispositions to disease, under the control and modifying influence of climate; for these are strongly inculcated, and riveted upon us by the dictatorial attributes of climate. Thus the same kind of occupations produce on the health of individuals different effects, when resorted to in different climates. Sedentary occupations, which produce on the inhabitants of northern districts, dyspepsia with all its concomitant train of symptoms, seldom if ever, disturb the health of those inhabiting southern districts; whilst active and laborious employments are illy suited to the latter, producing exhaustion, disease, and death, they are perfectly consistent with, and imperiously demanded for the well being of the former. That then, for it is this which constitutes the characteristic difference, of climate, would appear to me to be the remote predisposing cause of all diseases (save contagions) which prevail in warm regions; whilst cold, or the absence of heat predisposes to those of cold regions, when as in a former paper stated, taken in reference to our particular congenital predispositions.

Bilious fevers are the endemics, of southern countries, constituting two thirds, perhaps, of all the diseased actions of these districts. There has consequently been more written in relation to the causes, and the method of curing those diseases, than perhaps of any other class. And it may be said to be fortunate for mankind, that the various and conflicting opinions, in relation to these causes, appear to have but little influenced that practice which experience has proved to be the most successful. But among all the theories which have been heretofore advanced, none have been destined to enjoy more than an ephemeral existence, save that which assigns the remote cause to marsh miasma, or malaria. In a former paper published in the Transylvania Journal of medicine, I stated my unbelief in the existence of any such cause of disease, for the best, as I thought

and still think, of all possible reasons; namely, that we have no evidence of its existence. I say no evidence of its existence, in manner and form as assumed; for I must contend, that if a regular paroxysm of fever can and may be produced from the impression of causes of whose existence, we are certain, and with the "modus operandi" (on the living system) of which we are acquainted, it amounts to no evidence, indeed it would seem to be the height of credulity and absurdity, to infer the agency of a something which exists, so far as experiment is concerned, only in imagination.

But it is not my intention to retrace my former steps on this subject, but to confine myself to a few additional remarks in reference to the existence of malaria, as the remote predisposing cause of fever. For a very considerable length of time after the first promulgation of the Lancisian theory of fever, which contends for malaria as a "sine qua non" of bilious fever, animal as well as vegetable matter was considered entirely sufficient in a state of decomposition for furnishing this peculiar nondescript substance. But, from a series of well tested experiments, with a great many casual opportunities of discovering the fallacy of this opinion, the miasmatists have thought it proper and prudent to drop animal matter, and cleave with the more desperate determination to vegetable alone. A review of some of the evidences that animal substances, in a state of decomposition, are entirely innocuous, may perhaps not at this time be out of place.

Bancroft, in his treatise on yellow fever, informs us that "the church yard of the Saints Innocens, at Paris, situated in one of the most populous parts of the city, had been made the depository of so many dead bodies, that although its area contained nearly two acres, yet the soil had been raised by them eight or ten feet higher than the adjoining streets; and that more than six hundred thousand bodies, had been buried in it during the last six centuries; previous to which date it was already a very ancient burying ground." Many complaints having been made concerning the offensive smells which arose from this spot, and sometimes penetrated into the adjoining houses, and the public

mind having been greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendence of M. Thouret, a physician of eminence in Paris, and in two years he accomplished its removal. "The exhumations, says he, were principally executed during winter, but a considerable part of them was also carried on during the greatest heats of summer: yet, he says, no accident occured during the whole course of labour to disturb the public tranquility." The account of M. Berthi, in relation to the burial places at Seville, shortly after an epidemic visitation of the yellow fever, is alike conclusive of the fact, that no danger is to be apprehended from the effects of decomposing animal substances. He remarks "that in one of the burying grounds south-west of the city, ten thousand dead bodies had been buried; in two others seven or eight thousand; and in that of Triand about four thousand. The heat of the weather was considerable; and the ground of the burial places being clayey, was already cracked into wide and deep crevices, through which a fœtid odour exhaled, the result of the decomposition, which was going on among these heaps of bodies." Yet it appears from his statement that no ill consequences resulted from it there or elsewhere. M. Howard, in his work on Lazarettos, informs us that at Smyrna large numbers of dead bodies were daily left during the prevalence of the plague, unburied, and putrefying in the open air; yet no bad consequences resulted from it.

Those who are employed in boiling the blubber of the whale, which is often in a very offensive state, suffer no inconvenience, as regards their health, from this occupation. We are also told that large quantities of fish are sometimes used for the purpose of enriching the barren soil in some districts of country, which are thrown on the land to the quantity of twenty bushels to the acre, where they lie and decompose, without being at all injurious to the health of the inhabitants.

It appears evident from the foregoing, as well as many other well supported facts, that animal substances in a state of de-

composition do not furnish that peculiar substance called *malaria*, supposed to be the cause of our autumnal fevers. And to this opinion the most enlightened physicians, of the present day, seem to have subscribed. Consequently, vegetable substances alone are now considered capable, associated with a due degree of heat and moisture, to emit under decomposition the latent germ of fever.

It would be well here, I think, to consult all the lights we have, in regard to those principles, which chemistry informs us are to be found in animal and vegetable substances; some combination, of which must form this fever-generating poison, if it exist at all.

"Vegetable substances, though they are all distinguished from each other, by peculiar characters, present several circumstances of agreement in chemical properties. Oxygen, hydrogen and carbon, to which a certain proportion of nitrogen, is occasionally added, are their principal ingredients, and variations of these elements cause the great diversity which subsists among the products of the vegetable kingdom."* It would consequently seem, that some one combination of these gases must constitute malaria; but with a great number of these combinations is the chemist entirely familiar, and I would ask him whether he has ever known a case of bilious fever produced by the action of these gases. It is admitted that many combinations are irrespirable; but experience does not show that in any state of condensation, or dilution, they are capable of producing fever. I will not pretend to say that nature in her vast laboratory may not, or does not form combinations, or manufacture substances, of the existence of which the imperfection of our means of conducting experiments gives us no intelligence. But I would ask, where is the necessity of a resort of this kind, when almost every day's experience convinces us of the fact, that long exposure to the heat of the sun, together with the sudden application of cold to the surface, is entirely sufficient to produce all the phenomena of fever; and, as observed in a

* Henry's Chemistry, Vol. 11, Page 142.

former paper, these vicissitudes obtain, more or less, in our climate every twenty-four hours? But to proceed; the analysis of animal substances, according to Henry, page 217, shows them to consist of oxygen, hydrogen, carbon, and a large proportion of nitrogen. In animal substances, then, but one principle exists which does not in nearly the same degree in vegetable substances, namely, nitrogen; and this, it is scarcely necessary to remark, is not the least deleterious among the different gases, being wholly incapable of supporting animal life.

If I have succeeded in making it appear, from the various quotations in regard to the innocuous effects of decomposing animal substances, that these are incapable, under the most favorable circumstances, of producing fever, whilst they contain the same elementary principles, and among these, two of the most deleterious predominate, viz. carbon and nitrogen, I would, with all deference, inquire of the miasmatist, by what process of reasoning he arrives at the conclusion, that decomposing vegetable substances, under the same circumstances, are capable of producing it? What combinations can be formed of oxygen, hydrogen and carbon, when extricated from a decomposing mass of vegetable substances, beneath a burning sun and moist atmosphere, which cannot be formed of the same materials, extricated from a mass of animal substance, under like circumstances? Is nitrogen the redeeming principle which so controls and modifies the others, as to render them harmless? Then many vegetables possess the same.

I will not venture to say that the miasmatic theory of fevers is a delusion; but I do say that it is not tenable on philosophical principles. And indeed it seems to me to partake very much of that kind of philosophy, which among the ancients, gave rise to their views of the superintendence of so many gods, attributing every thing which befel them, whether good or evil, to the agency of some appropriate divinity, of whose divine self they appear to have known about as much as we know of miasma.

Since writing the above I have been favoured with the perusal of two papers written in opposition to the miasmatic theory of fevers. The one by Doctor Jones, of Augusta; and the other

by Doctor Ficklin, of Louisiana, published in the Transylvania Journal of Medicine, which augur well, and declare that at least the spirit of inquiry is awakened, and bids fair to test the stability of the miasmatic doctrine. But I must confess, that with neither of those theories am I altogether satisfied. That which considers moisture the sole cause is objectionable, inasmuch, as the author seems to have amalgamated the remote and exciting causes, in one and the same thing. That moisture under certain conditions of the system, applied to the surface, is capable of producing all the phenomena of fever, is readily admitted, and so far as I know has never been controverted; but that moisture is capable of producing a predisposition to fever, (especially when we consider the amount and length of time that we are usually exposed to it,) does not appear quite so clear. It is evident that that agent whose action on the system produces in it a condition favourable to the production of fever by the action of moisture, or any other exciting cause, is the remote predisposing cause. Now, inasmuch as Doctor Jones has said nothing in relation to the remote cause of fever, he appears to have left us to infer that moisture, according to his own language, "is the sole cause of fever." It is clear that the remote, as well as the exciting causes, are absolutely necessary in order to produce disease; for in vain shall Doctor Jones apply moisture to the skin in order to produce a predisposition in an individual to fever: and that agent which produces the different states of the "cutaneous apparatus, and muscular relaxation" of which Doctor Jones speaks, is essentially the remote predisposing cause; and under the same predispositon I am disposed to believe that many other causes would develope a paroxysm of fever with as much certainty as the application of moisture. I can therefore only consider moisture in the same light, in reference to the production of fever, with all other causes which can excite a pre-exciting predisposition into actual disease. I should have been pleased, had Doctor Jones been a little more specific and informed us what he considered to be the cause productive of that condition of the system favourable to the action of moisture in the production of fever.

Is it caloric? If so there is but littte difference of opinion, but with regard to the exciting causes, for in my former paper wherever I spoke of climate it was in reference to temperature, believing myself that exposure to heat is the remote predisposing cause of all our autumnal diseases. This opinion is not a little corroborated by the fact that it has become almost proverbial, that warm climates are less prejudicial to females than to males: and my experience goes to the support of the fact, there having always been, in my practice, a much greater amount of febrile disease among the males than females. The great number of widows, who are found in all southern countries, is no less conclusive. To what shall we ascribe this difference? Why should there not exist among females as strong a predisposition to disease as among males? If miasma create the predisposition, and as is contended, this is borne on the wind and interspersed through the atmosphere, would they not as well as males, inhale the poison and become as obnoxious to disease? Houses would not secure them against the inroads of the destroyer, nor would they protect them from exciting causes. But placed in the most airy and cool situations, with houses admirably constructed for the admission and free circulation of the air, in their avocations being almost entirely confined within doors, they escape that debilitating influence of the heat of the sun which predisposes men to febrile attacks, whom necessity or choice causes to pursue their labour, or sports, during the heat of the day.

But to return, as before remarked I am not content with that theory which ascribes the sole cause of fever to moisture, nor indeed with that which attributes it to carbonic acid gas, in different degrees of dilution. If it be a fact that the decomposition of animal substances is not injurious to health, then this theory must of necessity fall to the ground; and I believe that there will be but few found, sufficiently confident to undertake to controvert the many well authenticated facts found in the annals of medical science, confirmatory of this truth. According to the analysis of Gay Lussac and Thenard, animal substances are found to contain more carbon than those

derived from the vegetable kingdom.* Consequently the decomposition of animal substances should, according to this theory, be more deleterious than that of vegetables. Moreover, had this been the cause, amid the thousand experiments, that have been made with this substance, by chemists in all ages of the world, its fever-generating capacity would long since have been discovered. Nor am I aware that in districts the most unhealthy, for instance those situated near marshes and low grounds, eudiometrical examinations have discovered any superabundance of this agent. I would however remark, that I consider any attempt to assign the cause of fever to appreciable agents much more laudable, than that which would ask us to believe in the existence of a thing which could neither be shown, nor proved to exist.

Art. II. *Observations on Professor Cooke's Theory of Fever.* *By* Dr. David Glass, of Hopkinsville, Kentucky.

To the Editors of the Transylvania Journal of Medicine, &c.

Gentlemen:

In the first number of your Journal we have read with much solicitude and attention, an Essay on the Theory of Fever, by John Esten Cooke, M. D. Professor of the Theory and Practice of Medicine in Transylvania University.

The deserved and merited distinction of an author should never avert the strictest scrutiny and investigation of his doctrines, when their adoption will affect any portion of the community. We would here aver *in limine*, that it is a conviction of the importance of the subject alone, and not malevolence which prompts the following remarks. For, emanating from such a source, the doctrines taught in the Essay before us will

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* Henry's Chemistry, Vol. 2, Page 228.

not fail to exert a considerable influence over the medical practice, particularly of this western country.

The author not unlike most other pyretologists commences his Essay with an exposition of the absurdities contained in most of the popular theories, "endeavouring to overturn from the very foundation the antique edifice of Fever." It is not proposed to call in regular review the entire Essay, but briefly to notice such of the leading features of the theory, with their explanations, as appear at least doubtful.

The first position laid down in this theory, is "that the remote causes weaken the action of the heart. The principal of these are cold and miasmata." Although we would not deny, that cold may be an occasional exciting cause of fever, yet we think it probable, that it never is a remote cause in consequence of simply reducing the action of the heart alone. For, if so, the same effect whenever and by what cause soever produced, ought invariably to be followed by fever. That this is not the case, however, will appear evident when it is recollected, that syncope is not necessarily followed by fever; that one of the most prominent effects of digitalis purpurea is weakened action of the heart, yet that fever necessarily succeeds its administration no one has ever contended. But the question would seem to have been settled by the experiments of Dr. Currie. These furnish numerous examples, clearly evidencing that something more is requisite than weakened action of the heart, from reduction of temperature, to produce that state of derangement generally termed fever. The very subject of the experiment, quoted in the Essay, to prove that cold weakens the action of the heart, was for several successive days made to undergo similar experiments with no other than temporary inconvenience.

Cold, it is asserted, weakens the action of the heart long before the energy of the brain is lessened. If this weakened action be not the effect and evidence of lessened energy of the brain, it may be asked, what are the evidences of lessened energy, and in what manner is weakened action of the heart produced by the operation of this remote cause? A solution of these questions is not even attempted in the Essay; consequently,

if the theories of the ancient humoral pathologists are defective "in not showing how the disordered state or internal derangement is produced by the remote causes," this theory must be considered equally defective in failing to show how the action of the heart is weakened by the operation of cold.

Miasmata, the other principal remote cause, are said to produce "effects on the system, viz. darkness of the complexion and blackness of the blood, long before there is any diminution of the energy of the brain." A bare assertion, that this cause "affects the colour of the blood of every person exposed to its operation," is no rationale. And admitting that it invariably blackens the blood, it is a mere truism only. Therefore, this theory is as defective as that of Doctor Cullen; for, referring certain morbid appearances to a cause, without giving an account of the manner of its operation in producing them, is no explanation.

Thus has the author not only failed to show the manner in which these remote causes produce their alleged effects, "and thus step by step dispelling the gloom in which the subject is involved;" but he has rejected the only probable means of accounting for the production of such effects, by denying, "that the causes act on the nervous system primarily, and thus, diminishing the energy of the brain, produce debility." Hitherto we have considered it a generally received physiological maxim that all actions are the effects of agents operating primarily on the nervous system, and, when morbid, diminishing its energy; producing that state of a part, or of the general system termed debility. We are aware that there is some ambiguity in the expression, *energy of the brain;* and that the author would seem to understand by it, an energy which is only concerned in mental operations; but this is not the meaning intended to be conveyed by Cullen; on the contrary, it is evident, throughout his theory, that he intended it to mean *an energy appertaining to the whole nervous system.* And as the expression is used by the author in objecting to Cullen's theory, it should receive the same general acceptation which Cullen himself attached to it.

Notwithstanding the above denial, yet, in the paragraph directly preceding, the author maintains that stimulants "are un-

doubtedly often remote causes of fever; by exciting the action of the heart too high to last, they indirectly produce the debility which precedes fever." Is not the action here excited, a consequence of impressions made upon the *nervous system primarily* by those remote causes? Is not the consequent diminution of the energy of the brain the cause, that this highly excited action cannot last? And is not the debility which precedes fever, an evidence of this diminished energy?

Although not disposed to enter into a defence of Cullen's theory, yet we cannot forbear remarking, that notwithstanding the author objects to it for "excluding simulants from the list of remote causes," we are unable to discover any just ground for the objection; or any material difference on this subject between them. For, in paragraph seventy-six, of his "First Lines," Cullen *only* contends that the *direct* effects, produced by stimulants are not *of themselves* such as constitute fever; while, in paragraph ninety-seven, he maintains that among other causes, intemperance in drinking (stimulants we presume,) seems to be a remote cause; and by weakening the system, *may of itself produce fever.*

The "consequences immediately flowing from weakened action of the heart" are "diminution of the quantity of blood sent into the arteries," and "accumulation of blood in the vena cava and its branches." How this accumulation, as a consequence of weakened action of the *left side of the heart*, can be supposed to take place, without disregarding the structure and functions of the heart, lungs, and their great vessels, is difficult for us to conceive. For, admitting the validity of the position, that "black blood does not stimulate the left ventricle of the heart as scarlet blood does;" still it is not shewn, nor is it contended, that the blood when blackened by miasmata is rendered different, except in colour, from that ordinarily circulating in the veins; neither is it contended, that black blood does not stimulate the right side of the heart; it must therefore, though blackened, be considered still the natural and appropriate stimulus of this side. This conclusion accords too, with the quotation from Goodwyn; for it is clearly deducible from it, that the right side of the heart

regularly and uninterruptedly received the black blood from the returning cava, and transmitted it to the lungs; but these failing to perform their function, imperfectly decarbonized blood passed to the left side. And as the action of this ventricle became weaker, the blood accumulated in the *left* auricle, which became completely distended, before the contractions of the ventricle ceased. Here it is very evident, that the distention of the left auricle could not possibly have occurred, if the contractions of the *right* side had been equally weakened with those of the *left.* Therefore, it is perfectly obvious that blood when blackened could weaken only the *left*, not the right side of the heart; that the accumulation must consequently begin in the *pulmonary veins;* and asphyxia be the result, before accumulation could much affect the vena cava.

But it should be observed, that it is very far from being generally received as a physiological truth, that "black blood does not stimulate the left ventricle of the heart;" for, while Goodwyn, (upon the results of whose experiments the author rests this position,) maintains that dark blood paralyzes the left heart by its presence in the ventricle; Bichat contends, that it acts upon the heart by permeating its tissue, and enfeebling each particular fibre; and Dr. Kay, from a number of well directed experiments, with more propriety concludes, that the action of the heart is arrested by the suspension of respiration, and the consequent cessation of the function of the lungs. And that dark blood, so far from having any positively noxious influence, is capable of supporting the motive power of the heart. With respect however to conclusions drawn from the results of experiments made upon living animals, we are disposed to concur with Professor Caldwell, who says that "it is unsafe to draw from them any conclusions touching the natural functions of the same animals when entire and in a state of health."

That accumulation distending the vena cava and its immediate branches can be the effect of weakened action of the heart by *cold* is also doubtful. There is no evidence, nor is it probable, that upon the *heart exclusively* impressions are *primarily* made by cold. Perhaps the action of the *vessels* is reduced *sometime*

before that of the heart is weakened; for, in some of his experiments, Dr. Currie says he observed the heart beating with great steadiness and due force, when the pulse at the wrist was scarcely perceptible. If the action of the arteries be weakened previous to, or even simultaneously with that of the heart, no other can be the effect upon the circulation than a proportioned diminution of its momentum throughout the system.

In short, this doctrine of accumulation rests solely upon the supposition, that the same consequences result from mere *weakened* action of the heart, that are observed after entire *cessation* of the motion of this organ. Thus, the conclusion, that accumulation of blood in the veins is the inevitable and immediate effect of weakened action of the heart, is held "confirmed by what is observed after death: the cava and its branches are found distended with blood, the arteries empty. This is not the effect of disease: in all the vigour of life, if the action of the heart be stopped, the immediate consequence is the same." To corroborate this conclusion, the following quotation from Bell is introduced. "When the breast of a living dog is opened by taking away the sternum with the cartilaginous appendices of the ribs, the lungs are observed suddenly to sink, and afterwards the circulation of the blood and the motion of the heart to cease. In a little time after that, the right ventricle of the heart and the vena cava are swelled as if they were ready to burst." Although we deem it unsafe to draw general conclusions from experiments of this character, yet this example furnishes no grounds to infer that *weakened action* of the heart would be attended by such effects. That accumulation of blood in the large veins about the heart is generally observed *after death* will not be denied; but this state of the vessels must be considered as the effect of disease destroying entirely the motive power of the heart. But the better to test the validity of this conclusion, let us contrast the foregoing quotation with the following, taken from Goodwyn: "he attended carefully to the changes in the colour of the blood, and in the corresponding contractions in the left auricle and ventricle of the heart; and in all the examples observed, that when the blood which passed

into the left auricle was florid, the auricle and ventricle contracted strongly, and the circulation went on as in health; but when the blood began to put on a shade of brown, the contractions were diminished, and when it was black the contractions ceased, although the auricle was distended with blood." In the former of these quotations the only effect upon the vessels detailed is that produced by and usually observed after death, which, as already observed, has no bearing upon the subject. In the latter, the effects upon the vessels observed during the continuance of life only are detailed. Here the action of the heart is weakened; the effects of which are accumulation commencing in the *left auricle*, and asphyxia and death from pulmonary engorgement. If the condition of the vessels had been observed in a little time after this event, they would have exhibited accumulation in the right auricle and systemic veins—a condition usually observed after death, and probably never occurring before, except in cases of malconformation, or lesion of structure of the heart or lungs.

Therefore, *weakened action only* would be attended by preternatural fulness of the *pulmonary veins;* and *entire cessation of action* by accumulation in the larger systemic veins. And this view is confirmed by the facts detailed in the above quotations. For, in the former, entire cessation of the action of the heart is attended by accumulation at the right side, while in the latter, engorgement of the *left* auricle is a consequence of *weakened action.*

Admitting however, that weakened action of the heart is a cause sufficient to induce accumulation in the vena cava and its branches, yet the following explanation of the manner in which it is said to produce the hot stage of febrile paroxysms is far too mechanical to give satisfaction "in an age in which the Newtonian method of philosophising" is adopted in the investigations of the laws of organic life. "By the continued arrival of blood, the accumulation distends successive portions of the cava, and of those of its branches which are unprotected by valves; and the distension of successive portions, rapidly made, has the effect of a current running back from the heart. As soon as the

distending fluid reaches the valves which limit the veinous cavity, they are raised; and this backward wave is instantly arrested, rebounds, and the impulse is directed towards the heart." It is clearly evident that no such effect can be produced as a wave or current running back from the heart, while the blood continues to pass through the right side of this organ. For, however languid the circulation, there must, during its continuance, be a progressive motion towards the heart of all the blood contained in the vena cava and branches, how much distended soever they may be. Add to which, it would seem to involve a physical impossibility, by supposing that two opposite motions can exist in the same column of fluid at the same time. Nor could this backward wave of the distending fluid, upon reaching the valves, *raise* or close them; because the force with which the blood could regurgitate, must be less than that which propels the blood through the valves, towards the heart. Of this any one may be convinced by this simple experiment. Make permanent compression on a large vein of the arm, and no valve will be seen to raise; the accumulating blood will rapidly distend successive portions of the vessel, until interrupted by anastomosing branches sufficient to transmit it towards the heart. But pass the finger firmly down the arm upon the course of the vein, and valves will be discovered to rise; because the force, giving the retrograde motion to the blood, is greater than that by which it is propelled towards the heart. Further, it is not probable that an accumulation much distending the veins could be *arrested* by their valves. For such a distention must render them too small to close up the caliber of the distended vessels. Again, this backward wave, upon reaching the valves could not *rebound* upon the heart, and force it to *increased action*. For, is not the momentum with which the blood is supposed to rebound, a second reflection of the force which originally propelled it through the valves upon the heart? If it be, (as every one must acknowledge) then, to suppose it capable of producing increased action of the heart, is supposing a reflected force greater than that of which it is a reflection; a proposition to which the author himself, we are persuaded, is not prepared to assent.

But the objections to this theory rest not here; for *accumulation* in the cava, with its consequent *backward wave* and *rebounding* can never produce an action of the heart greater than its natural, or healthy action. Consequently morbidly increased action or fever would never supervene without the intervention of some other cause. To supply this *dissideratum*, increased excitability is said to be acquired by the heart during its weakened action caused by a deleterious agent! Representing an organ of involuntary motion, while labouring under the influence of such a cause, to be in a condition similar to the muscles of voluntary motion, while in a state of rest, is extending the "inductive mode of philosophising" rather beyond the bounds of probability; it is not confining it to that of rigid inference from known facts. While the heart labours under the direct influence of a cause (black blood for instance) weakening its action, it cannot be considered in a state of rest. And though its efforts be insufficient to transmit or remove the accumulation, it will maintain an action commensurate with its energy; which energy never will increase, until the cause lessening it is removed. But what is this *feeble action*, and what this *renewed power to act*, other than the debility, from abstraction of stimulus, and the increased excitability of Darwin and Rush? A defect in whose systems, the author has himself so successfully exposed.

P. S.–The preceding observations were written several months ago; but owing to unavoidable circumstances they have been kept back until the present late period. D. G.

June 15*th* 1829.

ART. III.—*Thoughts on Sedatives*, By CHARLES CALDWELL M. D.

IN recognition of its preeminence, in its modes of pursuing knowledge, the present is often denominated the "age of fact and experiment" as contrasted with former periods, which are supposed to have been more addicted to theory and abstract

discussion. To be regarded as having done any thing useful, and be deemed worthy of respectful notice, an inquirer now must adduce some new fact, or discover some new being or substance; and whoever does this is sure to be proclaimed a promoter of science, and a benefactor of his race, whether the thing discovered be important or not. Nor, with ordinary thinkers, can he attain to such distinction in any other way, however profound may be his reflections and acute his abstractions; or however powerfully and correctly he may reason, and judge.

This passion for novelty is universal. To be enrolled among the improvers of his profession, the botanist must discover a new plant, the zoologist a new animal, the chemist a new substance, the physician a new remedy, and the warrior a new mode of despatching his enemy. Even the mechanician, if he does not discover a new power, must invent a new name for some combination of powers already known, or, however great his merit, his own name will be as perishable as the most fragile materials on which he works.

To this burning love and spirit of discovery no one can except. In themselves they are highly praise-worthy and useful. They constitute, in no small degree, the promise and glory of the age. They have already done much, and are destined to do still much more, in promoting science, elevating the character of man, augmenting his comforts, and adding to his happiness.

But it is possible that, like other things, they may be misdirected and abused. They may be confined too exclusively to a single channel, and to a few objects, to the neglect of others of equal or greater importance. That this is even the case, in the present instance, I feel myself scarcely permitted to doubt. The press after *new facts*, and *new kinds* of substance or entity, is too exclusive and absorbing. It diverts the minds of inquirers from other objects of superior moment in the development of science. I allude to *principles* and *laws* which are too much overlooked. Without these, facts would be chaos, and science but a name. For science is facts arranged into principles. Nor is it requisite or important, in the formation of principles, that

the facts should be new. Those that have been known for ages are equally valuable with such as were discovered but yesterday.

The mere experimenter and fact-hunter, then, is far from being the only, or even the most essential, contributor to science. Of a much higher order are the contributions of him, who collects and embodies facts, whether new or old, compares them, ascertains their place and relations in nature, classes them, constructs them into principles, discovers the laws by which they are governed, and thereby masters them, and gets them under his control. He is the individual whose knowledge is real power, because he can wield it at pleasure, and bring it into action for the accomplishment of his purposes. He moves facts in numbers and masses, and makes them act conjointly, like a skilful commander, at the head of a disciplined army. Hence he is efficient and successful. But the mere fact-gatherer acts with them singly and, therefore, very feebly, like a factionary at the head of a disorderly mob, where all is misrule, and each individual acts without system, or concert with his fellows. Such an inquirer is necessarily confined within a limitted sphere, and can never, of himself, extend the boundaries of any science. The facts which he may discover must pass through the hands of a superior workman, or they will never find their place in the temple of science, or be productive of the advantages of which they are capable.

It is not true, then, that to become a distinguished promoter of any branch of science or art, an individual must be necessarily a discoverer of new facts in it. He may operate to equal effect by working with facts already known. Of this we have many memorable examples.

In the construction of the telescope Gallileo discovered no facts that were actually new. He availed himself of a combination of those that were already familiar to him. The same is true of Newton, in his discovery of gravitation, the principles of astronomy, and the laws that govern the heavenly bodies. His object was not the ascertainment of new facts, but the classification of such as were already known to him. It was the discovery of principles and laws through the medium of facts.

Of steam-navigation the same thing may be affirmed. In effectuating it, Fulton discovered no new fact in mechanics. He combined such as had been known for ages.

In taking a retrospect of medicine, we find that some of the most celebrated improvers of that science have enriched it with but very few facts of their own discovering. They have laboured in a higher sphere, and directed their attention to more difficult and important objects, the classification of facts, the discovery of the principles on which the profession is founded, and the laws by which the administration of remedies is governed. Of this, were it necessary, many distinguished examples might be adduced. As the prime duty of the chief of an army is not to use his own sword in battle, but to combine and direct the military qualities and movements of his troops, so, in medicine, the more highly talented often employ, to effect their purposes, the labours of others. Nor do they deem facts the less valuable, because they have not been discovered by themselves. If their own observation has verified them, they hold them authentic, and employ them as means in the developement of principles, and the discovery of laws. Provided they can discover new relations between old facts, and apply the facts themselves to new purposes, they are content with their labours. Nor does this betray in them a want of ambition. To place medicine under the control of reason, and to unfold the principles on which it is founded, and the laws by which it is governed, as a source of practice, is an enterprise sufficiently arduous and honorable to satisfy the loftiest aspirations of philosophy. Nor is it extravagant to allege, that, in time to come, more numerous and important improvements in medicine will arise from new modes of viewing and employing existing facts, and remediate articles already known, than from the discovery of new facts, or further additions to the materia medica.

Let it not be imagined, from what has just been said, that I think lightly of either facts, the search after them, or those who anxiously and ardently pursue it. Such a belief would be most unjust. I trust I know the value of facts, and place the

true estimate on them. They are at once the foundation and the material of science. But they do not alone constitute science. To rear the fabric, and give to it symmetry, beauty, and durability, principles are essential.

Nor, as already intimated, are the powers exercised in the mere search after facts, of as elevated an order, as those employed in the evolution of principles. Facts may be discovered by accident. They are often discovered also by men of very limited intellects. But, as relates to principles, the case is different. The discovery of them is the result of comparison, reflection, reason, and analysis; and belongs to intellects of a higher order. The common hunter, in the forest, or the angler, by the stream, may discover a new quadruped, bird, fish, or plant; but the philosopher only can arrange in classes those already known.

Believing the preceding observations to be true, I shall proceed, without further preface, or any apology, to the discussion of a subject, which I deem interesting, although I do not profess to be in possession of any facts relating to it, which are not perhaps known to most of the cultivated members of the profession. I allude to the doctrine of what are denominated *sedatives*. My object is to endeavour to ascertain, by a fair but brief analysis of the subject, whether there exist a class of remedies, which, in strictness of technical language, ought to be thus denominated. Or whether, on the contrary, the term is not one of the numerous misnomers, which have arisen from the want of a correct knowledge of the principles of living organized matter, and the laws by which its action is governed.

Shall I be met in the very threshold of this inquiry, with the oft-repeated remark, that the discussion of sedatives and stimulants is but a controversy about words, destitute of usefulness, and unworthy of attention? I reply, that the allegation is unfounded. The controversy is not about words, but about the true relations of certain agents to living matter, and the principles on which they produce their effects on it. The discussion, then, must necessarily involve the consideration of those fundamental properties and laws of the animal economy, through

which it is susceptible of impressions from without, as well as from its own internal affections. It involves, in fact, an analysis of the real condition and character of man, as a living organized and sensitive being, and the relations he bears to nature around him. It is, therefore, an important point in the philosophy of medicine, and of living matter, fully worthy of the attention and powers of the most highly gifted and extensively informed individuals of the profession.

The question to be considered is the existence or *non-existence* of a class of agents that may, in strictness, be termed sedatives? Does such a class constitute a portion of the materia medica? or ought the term to be stricken from its nomenclature?

Before a reply to these questions can be attempted, with any prospect of success, it is requisite that the meaning of the word sedative be correctly understood. The term is known to designate a class of medicinal agents, whose effect is supposed to be a *direct diminution* of the force of action in living beings, without previously augmenting it, or, in any other way, affecting its character. If it alter the *nature* of the action, it is not a sedative, but a stimulus different from that whose effect it has changed. That the character of the sedative may be complete, it must produce no excitement in any portion of the system on which it acts, if indeed it can be said to act and yet do nothing. The diminution of action which it produces must be local as well as general, in the part to which it is immediately applied, as well as in remote parts, or the entire system, otherwise it has no claim to the name of a sedative.

To speak in terms still more explicit, and I trust more clearly and certainly intelligible. A medicinal substance is applied to a certain organ or part of the human body. Let it be the stomach or skin. In the stomach or skin, as well as in the apparatus of circulation, and other parts of the system, it produces *directly* a dimunition of action, and nothing else. In no respect, except by diminishing it, does it change the action. It is a *sedative.* But if it increases the action in the organ or part to which it is applied, or essentially alters its nature, no matter what may be its effects on other organs, it is not a sedative. It stands re-

lated to the system as a stimulus, as will be made to appear more fully hereafter. Should it simply diminish the action of the part which it touches, while it augments the force of action, or changes its nature, in other parts, it is still a stimulus. To be really a sedative, it must diminish exclusively, in every organ to which its influence reaches. A stimulus only can prodcuce excitement either locally or generally, primarily or remotely.

If this definition of a sedetive be erroneous, I shall feel gratified and obliged by having it corrected. If true, it is the proper test by which to determine the nature of agents, in the present discussion. In full belief of its truth I shall proceed in my inquiry. But I shall first submit to the reader a few propositions, which may be regarded as axioms in medical science; and which have an essential bearing on the present question.

1. All vital action or excitement is the product of stimulation. In other words, it is the result of a stimulus applied to, and acting on, living matter.

2. Excitement in muscles, capillary blood-vessels, absorbents, and the skin, is manifested by motion, in the nerves by sensation, and in the brain by some kind of intellection or thought.

3. Whatever, then, in the human system, produces motion, sensation or thought is a stimulus. This is universally admitted.

4. Every stimulus, of sufficient power, when applied to the human system, produces a two-fold action, a primitive and a secondary, or an original and a sympathetic. The primitive or original appears on the spot to which the stimulus is applied, and the secondary or sympathetic in some remote organ, or in the whole system.

5. Of these two kinds of action the primitive is that by which alone we can correctly determine the nature of an agent, and its true relation to living matter. In plainer terms, it is by its action on the part with which it comes into immediate contact, that the question can be determined, whether an agent is a sedative or a stimulus? This truth may be deemed self-evident. If the agent excite the part, by either augmenting its action, or altering the nature of it, it is, as already observed, a stimulus. If it simply diminish its action, without altering it, it is a sedative.

6. As relates to the solution of the question I am considering, the secondary or sympathetic action is of no avail. It will appear hereafter, that by assuming it as the test of the sedative or stimulating character of agents, the utmost incongruity and confusion will ensue. The same substance will be, at different times, in different quantities, and under different modes of administration, both a sedative and a stimulant; a position, than which none can be more fallacious and absurd. I may correctly add, that under the decision of this test, every agent, not excepting caloric itself, can be proved to be a sedative. And such it has even been pronounced to be.

7. The nerves furnish as fair a test of sedative and stimulant action, as the muscles, capillaries, absorbents or skin. Whatever excites them is necessarily a stimulus.

The truth of these positions no physiologist, I think, will deny or question. Nor will their pertinency to the discussion I am engaged in be held in doubt. Let us then test by them some of the reputed sedatives of those who maintain the sedative doctrine. That the trial may be fair, and, if possible, conclusive, I shall begin with *cold*, which is the strong hold of the advocates of the doctrine referred to. If I can prove that to be a stimulus, their citadel is carried, and their outworks will fall, as a matter of course. By cold I mean, in the present instance, the relation which a substance, of a lower temperature, bears to the human body possessed of life, or to any other kind of living matter.

Shall I be again told, as I sometimes have been, that cold, as here defined, consisting in an ablation or absence of caloric, is a *negation* of being, and cannot, therefore, be productive of *positive* action? and, hence, that it must, of necessity, be a sedative?

I reply, that, in the present inquiry, it is not either my province or my purpose to treat of doubtful entities, or to bewilder myself and the reader with subtle abstractions. It is the same to me whether the sensation of coldness arises from the passage of caloric out of the body or into it. The sensation and its concomitants are produced, and do exist, and that is sufficient for

the objects I have in view. My intention, therefore, is to speak of physiological and pathological phenomena and their causes as connected with living matter, not to toy with chemical subtleties, of which I profess to know but little, of which no one knows, in reality, any more, and which have no bearing on the subject in question. Holding these things in remembrance, I shall proceed in my inquiry.

It has been already intimated that, as relates to the action of cold, the human body may be divided into two branches, the *moving*, and the *sensitive;* or that in which action is accompanied by a perceptible change, and that where it is manifested only by sensation. If, in the former of these, cold produces motion, and, in the latter, sensation, it is essentially a stimulus. Let the experiment be made, and the result noted. It is sufficient for my present purpose, to include under the first branch, the skin, muscles, glands, absorbents, and capillary blood vessels. The second consists of the nerves and brain.

Of the Skin.—Cold acting on the skin produces obvious contraction. This appears in paleness, which arises from a contraction of the capillaries forcibly emptying themselves of their blood, and that condition of the skin called *cutis anserina*, which seems to be a genuine contraction of that membrane. But these phenomena are the result of stimulation, and can be the result of nothing else. To prove this, they can be produced by agents, which are universally acknowledged to act as stimulants, and to be capable of acting in no other way. Apply to the surface of the body water of a temperature so high as to burn. Its first effect on the skin is contraction and paleness, precisely like that arising from cold. But the contraction soon ceases, the blood returns in preternatural abundance, and the part becomes inordinately red. Let this experiment be performed under accurate inspection, and the issue will be found to be as here represented. But hot water is certainly a stimulant.

Apply to the skin the point of a scalpel, gently pressing it without producing a wound, and mark the effect. For some distance around the scalpel the surface will be found to be bloodless and contracted, precisely as if it were under the influence of

cold. But it will scarcely be denied that pressure by an edge-tool or a sharp pointed instrument is a stimulus.

Apply cold to the scrotum. The result is an immediate and striking contraction of the skin, dartos, and the other membranes of the part. Apply to the same part suddenly heat intense enough to burn, and the effect will be the same. The contraction will be obvious and even considerable. Irritate the part with a pointed instrument, it will in like manner contract. If then heat and the pressure of keen edges and sharp points can thus produce contraction by a stimulating agency, it cannot be admitted, and will not, I think, be contended, that cold will do the same when acting as a sedative.

But perhaps the most memorable instance on record of the contraction produced in the skin and capillaries by cold, occurred in the person of Dr. Solander. That enlightened and enterprising physician is known to have accompanied Captain Cooke, in his first voyage of discovery around the world. When lying at Terra del Fuego, the Doctor, at the head of a select party, made a botanizing excursion across some of the mountains of that inhospitable island. They were overtaken by a snow storm, in which they were all endangered, and two or three of them perished. The Doctor himself was very near being one of the unfortunate number. With the utmost difficulty his companions succeeded in conveying him to the ship. The effect of the intense cold on the whole exterior of his body, but especially on his extremities, was surprising. So great was the diminution of his feet and hands, in consequence of the contraction of the skin and the capillaries forcing the blood out of them, that his shoes and gloves repeatedly fell off. Had the cold operated as a sedative it could not have done this. Nothing but a very powerful stimulus could have thus acted.

Of the Muscles.—The peristaltic motion of the bowels consists in muscular action. But it is well known that that can be often excited by injections of *cold* water much more readily than in any other way. Hence, other agents having failed, such injections have produced evacuations, and saved life. At the same time that the cold has thus awakened muscular action,

it has excited copious intestinal secretions, which are also the result of a stimulating impression.

When, in hot weather, the system is greatly excited by severe exercise, an excessive draught of *cold* water often produces in the stomach a violent spasm which proves suddenly fatal. In such cases the contraction of the stomach is so great, that the organ has been found, after death, diminished to a third or even fourth of its ordinary size. I need scarcely add, that by a sedative agent no such effect could possibly be produced.

It is many years since the following decisive experiment was performed.

The heart of an animal was exsected before it had ceased to palpitate. Three cups of water had been provided. Of one cup the temperature was near 200° of Fahrenheit. Of another the temperature was about 35°, while that of the third was the same with the temperature of the heart; say about 87°.

While the organ lay exposed on a table, the hot water was dropt on it, when it immediately and strongly contracted. The cold water being dropt on it, its action was the same, except that the contraction appeared to be stronger. But when the water possessing its own temperature fell on it, its movement was so feeble as to be scarcely perceptible. Sometimes it was not perceptible at all.

This experiment was carried to the extent of satisfying every one who witnessed it. It spoke a language which could be neither misunderstood or gainsaid. It proclaimed the stimulating action of cold, under a force of evidence which nothing can resist.

Of the Glands.—The effect of a draught of cold water taken into the stomach, and of cold water and cold air applied to the skin, in exciting the action of the kidnies, is known to every one. Those agents are among the most powerful diuretics that can be employed.

A horseman travelling in cold weather, discharges, in a given time, an unusual quantity of urine. Cold water dashed on the skin, especially on the lumbar region, or the lower extremities, produces often suddenly a superabundant secretion of the

same fluid; and a moderate draught of iced water has acted more powerfully on the kidnies, than all the most famous diuretics of the shops. In the evening of his life, Dr. Franklin suffered much from an obstinate torpor of the urinary organs. The most certain and effectual relief he experienced from that complaint arose from his standing barefooted on a *cold* marble slab, which he kept in his chamber for that purpose.

Cold applied to the skin increases oftentimes, in a high degree, the secretory action of the internal surface of the alimentary canal. Hence its effect on intestinal evacuations. A piece of ice held in the mouth, produces a copious secretion of mucus and saliva. Apply ice or snow to the eyes and a preternatural secretion of tears will be the consequence. Introduce the same substances into the nares, and the action of the Schneiderian membrane will be increased.

Of the Absorbents.—In the diminution of the volume of the feet and hands, by the influence of cold, the action of the absorbents is necessarily concerned. By the impression made on those vessels by the low temperature, to which they are exposed, both interstitial and cellular absorption, in the parts, is obviously augmented. So are the same kinds of absorption, in certain tumors, the removal of which is often accelerated by the application of cold. These points are too well known and settled to be dwelt on. To the enlightened reader a mere reference to them is sufficient. In promoting digestion, which a draught of cold water is known often to do, it probably produces the effect by augmenting both lacteal absorption and gastric secretion, as well as by strengthening some unknown mode of vital action in the mucous lining of the stomach. But all this is the result of stimulation. To speak of it as the issue of a sedative influence, is an abuse of terms.

Of the nerves and brain.—The stimulating effect of cold on this branch of the system, is, if possible, still more palpable. To deny it would seem to comport with any thing but experience, and to participate of any thing but common sense. Who that has ever felt cold can doubt it? It may be regarded as a primitive truth, in its effect on the mind immediate, universal,

and irresistable. It is doubtful whether discussion can either illustrate or strengthen it. Whatever produces sensation, whether pleasurable or painful, is a stimulus. By that test let the question be decided.

A certain degree of coolness is a source abundantly rich in some of our choisest gratifications. Who, when languishing under dog-day heats, has not experienced the pleasure of a cooling breeze? or who, when thirsty and fatigued, or in a high and burning paroxysm of fever, has not felt himself at once delighted, revived, and invigorated, by a draught of cold water?

The hands, feet, and face are exposed, without a covering, to the temperature of zero, or ten degrees below it. The sensation produced becomes, in a short time, insufferably painful.

An individual has two carious teeth, with apertures penetrating their cavities. Into the aperture of one of them he introduces a spicula of ice, and a red hot wire into that of the other. The sensations produced will be eminently painful, and so much alike that one cannot be distinguished from the other. As well, then, may heat be denominated a sedative as cold.

Two individuals are lying asleep in the same apartment. Into the bosom of one of them is introduced a ball of snow or ice, and into that of the other a mass of iron heated to the temperature of 150° of Fahrenheit. Both will start from sleep at the same instant, and remove with equal eagerness and promptitude the offending substance.

A mass of quicksilver is reduced, by cold, to a state of solidity, in which case it is at the temperature of 40° below zero. An equal bulk of iron is heated to the temperature of 235°. In one hand of an individual blindfolded is placed the quicksilver, and in the other the iron. Each substance will be dashed away, at the same moment, by the person experimented on, and the sensations produced, being exquisitely painful, will be pronounced in their character, perfectly identical. The impression made by heat will not be distinguishable from that made by cold. This experiment I have seen performed repeatedly, and its issue was always the same. The quicksilver will blister the part it touches as certainly as the iron.

About the middle of the eighteenth century a party of French Savans went, by appointment, to measure a degree on the meridian, within the northern polar circle. In that inhospitable climate they passed the long and dismal night of winter The cold was most intense, their sufferings were severe, and the record of them which they kept was interesting and instructive. When they ventured out of their hut, the impression made on their faces, by the frosty air, was like that of a keen cutting instrument. When their guns, sabres, pistols, or pieces of their apparatus, had acquired the temperature of the atmosphere, they often *blistered* their hands, like highly heated metallic substances. The sensation and effect which they produced, on being brought into contact with the skin, were, in all respects, like those of a severe burn. To say that these were not the effects of a sedative influence would be superfluous. Nothing could produce them but a stimulus as genuine and powerful as cantharides or caloric.

Many other well known phenomena testify, in a manner equally conclusive, to the stimulating agency of cold.

From loss of blood, or the influence of lightning or carbonic acid gas, an individual is reduced to a state of asphyxia. The most powerful and successful resuscitant is *cold* water springled or dashed forcibly on the face, and surface of the body. In these cases, volatiles acting on the olfactory nerves, and stimulating injections, although salutary remedies, are much less efficacious.

Before I was yet a practitioner of medicine, a memorable instance occurred to me of the stimulating effect of cold water. It was in the person of a fellow student and friend. He was preparing to write his Inaugural dissertation on the deleterious influence of carbonic acid gas. Making himself the subject of his own experiments, and continuing too long exposed to the action of the gas, he fell down in a state of deep and alarming asphyxia. From this, after other remedies had been resorted to in vain, he was aroused by the stimulus of cold water forcibly dashed on various parts of the body.

The history of the Grotto *del cani*, in Italy, is known to every

one, who has either traveled himself, or read the travels of others, in that country. The grotto is full, to the height of two feet above its floor, of carbonic acid gas. A dog is placed within it, exposed to the action of the gaseous poison. The animal soon falls down in asphyxia, and is then taken up and plunged into lake Avernus, which is just at hand, and whose waters are of a very low temperature. The consequence is an immediate resuscitation by the stimulus of cold.

So powerful and peculiar is the stimulating action of cold, that water at a low temperature, judiciously applied to the surface of the body, has been found, by many recent experiments, to be the most effectual remedy to break the torpor induced by inordinate doses of opium. More than twenty years ago, when I was engaged in investigating the action of cold, two cases illustrative of this occurred in my practice, and were briefly noticed in one of the periodicals of the day. I aroused, from the stupor of excessive opiates, two patients, by dashing cold water on the face, neck, and breast. But I did not pursue the the subject to the requisite extent. Of late it has been ably and satisfactorily treated, especially by Dr. Cross, formerly of Lexington, now of Alabama.*

Another instance in which the stimulant nature of cold is very strikingly manifested is the cold bath. Whether we have respect to the plunging or the shower bath, the amount of the shock received is in the inverse ratio of the temperature of the water. The colder the water, the severer the shock. But to predicate of a sedative the power of giving a shock, is to employ terms that are self-contradictory. The capability of shocking belongs only to electricity, galvanism, and other stimulants of great activity. The very term itself is conclusive of this. *Light darkness* or *hot coldness*, would not be a more incongruous expression than that of a sedative shock.

Ophthalmia is often aggravated by cold air and cold water. So is inflammation in the case of wounds. Hence, for the castration and spaying of their domestic animals, judicious and

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* See Transylvania Journal of Medicine &c. No, IV. Vol. I.

practised agriculturists always select weather of moderate temperature. So do surgeons of reputation for the performance of important operations, when circumstances allow them the opportunity to choose. Of this selection experience is the source. It testifies that wounds of every description are much more prone to inflammation during very hot and very cold, than during moderate weather. The reason is obvious. Heat and cold are equally irritants; the latter no more of a sedative than the former.

A marmot, hamster, or any other hybernating animal is lying torpid in an atmosphere at the temperature of 45°. Remove it to an atmosphere of 80° and it will be, in a few minutes, resuscitated. So will it, with equal certainty and celerity, by being transferred to an atmosphere at the temperature of zero. In either case it is stimulated; in the one by heat, in the other by cold. This is the only explanation of the phenomenon which reason sanctions.

An individual is exposed to-day to an atmosphere at the temperature of 80°. To-morrow he is exposed to one at zero. In the latter case his system secretes, or otherwise evolves, in a given time, a much greater amount of caloric than it does in the former. For this there must exist a physical cause. Nor is there, perhaps, any one to which the phenomenon can be so reasonably referred, as the powerful and peculiar stimulus of cold, acting on the skin. That stimulus, constituting an item of the universal fitness of things, excites the system of man and other animals to the degree of action requisite to the maintenance of its healthful temperature.

A full dose of opium, a bottle of wine, blisters, sinapisms, strong emotion, and other stimulating agents, judiciously managed, have often prevented paroxysms of intermitting fever. So has exposure to intense and pinching cold. Nor does the effect seem attributable to any thing else, than the powerful impression made on the system by an inclement atmosphere. And every impression is necessarily a stimulus. The phrase "sedative impression" is as self-contradictory as "sedative shock." As forms of expression they are alike inadmissable.

Shall I be told, in answer to all this, that cold, *long applied* to the surface of the body, weakens the action of the heart and the muscles of voluntary motion, takes from the energy of the nerves and brain, and impairs the powers of the whole system, and must therefore be a sedative? I reply, that the fact is true, but the inference is not.

If cold is a sedative, because an *excessive dose* of it diminishes the powers and efficiences of the system, every thing else is a sedative, for the same reason. This is as true of wine, opium, alkohol, electricity and even caloric, as it is of cold. Each of those articles, acting on the system, in excessive quantities, diminishes its powers, and thus produces what may be denominated a *sedative effect.* And cold does the same. But it is only when applied in excess that it produces such an effect. In a moderate and suitable dose, *it always stimulates*, precisely as is the case with the other stimulating agents referred to. If I mistake not, a brief analysis will illustrate and explain this topic, to the comprehension and entire satisfaction of every one.

A moderate dose of opium stimulates and invigorates; an excessive one debilitates and often destroys. Sparks of electricity irritate and excite; but a stream of lightning from the heavens overwhelms and prostrates. Of wine and alkohol the same is true. In moderate quantities they elevate action, and confer strength; in excessive quantities they diminish the former and detract from the latter. Nor is it otherwise, as relates to caloric itself. A slight burn is an irritant and produces fever; but a very extensive and severe one prostrates the system below re-action, and no fever can ensue. An icy and irremediable chilliness prevails, and the pulse sinks, becomes tremulous, and stops. But a slight burn arises from a moderate dose of caloric, and a severe one from an excessive dose.

Such, in different quantities, is the modified action of the most potent of the agents acknowledged to be stimulants. But their *real nature* suffers no change. They are the same when they depress and even extinguish action, as when they elevate and strengthen it. They are always, essentially, and immutably stimulants.

As relates to cold the same is true. The parallel between it and the other articles referred to is perfect. A moderate degree of it excites and gives tone. A high degree depresses and exhausts. In this its nature is unchanging, and its action uniform. If cold be a sedative, then, so is every thing else that is capable of producing an impression which can be carried to excess.

By a fair analysis it may be conclusively shown, that, as relates to all reputed sedatives, the doctrine generally prevalent is unfounded. They prove sedative only *indirectly*, in their remote consequences, not in their immediate action. Their primitive and essential relation to the human system is that of stimulants; and they depress action only on secondary grounds.

There belong to the human body, in a living and healthy state, three properties which serve as channels for the introduction and propagation of excitement and action. These are irritability (the organic sensibility of Bichat) sensibility (the animal sensibility of the same writer) and sympathy. The two former of these are primitive, the latter secondary. Extinguish the two former, no topical impression can be made on the system. Extinguish the latter, no topical impression can become general. Sympathy, then, is the sole ground of general excitement.

In a higher or lower degree, irritability and sensibility belong to every organ of the body. Sympathy is the living and universal bond, which binds together the whole of the organs, constituting the system a *vital unit.*

Irritability and sensibility form the outposts of life, receiving all *primitive* impressions. Sympathy watches over the interior of the fabric, and is concerned only in impressions that are *secondary.* It is obvious, then, that the immediate and most essential relations of agents producing impressions are to irritability and sensibility. Hence those two properties are necessarily the true and only tests of the nature of every thing whose action they receive. They alone, therefore, ought to be *always* consulted; sympathy *never.* They first sustain impressions, the subsequent manifestations of sympathy being modified by their character.

If these propositions and principles are correct, as I feel persuaded they are, their application would seem capable of settling satisfactorily the doctrine of sedatives. Let the experiment be tried.

As already mentioned, irritability and sensibility receive exclusively all original impressions. Through no other channels can such impressions gain admission into the system and awaken sympathy. But, in the nature of things, it presents itself as a primitive truth, that irritability and sensibility can be impressed only by stimulating agents. Of nothing else are they susceptible. Sedatives, did they exist, could bear to them no practical or conceivable relation. Unless awakened by impression, the properties are dormant. But a sedative has no aptitude to awaken from torpor or inaction. On the contrary, its tendency is to produce them. This point is settled by the definition of the term. To have the power to arouse, an agent must be a stimulant. By the aid of sensibility and irritability let some of the reputed sedatives be examined.

Tartar emetic.—Taken internally this is accounted a powerful and direct sedative. But the notion is fallacious. To the irritability and sensibility of the stomach, to which it is applied, tartar emetic is a very powerful stimulant. Perhaps it had better be denominated an irritant. To the skin also it is a very active irritant, producing on it inflammation and pustules of a peculiar character. It is also efficacious in promoting secretory action, an effect which it produces only by its stimulant agency. By sympathy only does it reduce the action of the heart, and give rise to muscular debility.

Digitalis.—This is pronounced a sedative, because it can be made to diminish very strikingly the force and frequency of the contractions of the heart. But this it never does until it has strongly excited the irritability and somewhat the sensibility of the stomach, with which it is in contact. The latter effect bespeaks it a stimulant. So does its operation in promoting the secretory action of the kidnies. It acts, moreover, as an irritant to any tender and sensitive organ. Introduce into the eye a drop or two of a decoction of digitalis, and it will irritate

it severely. Apply it to the skin denuded of the cutis, and it will produce inflammation. Taste it, and it is bitter and somewhat pungent. All this, I say, proclaims it a direct stimulant. Its sedative action on the pulse is sympathetic and secondary.

Of cicuta, and the entire class of what are denominated anodyne sedatives, the same is true. Before they reduce the action of the heart, they irritate the stomach, producing in it unpleasant and morbid sensations. They can also be made to excite inflammation on a portion of the skin deprived of the cuticle.

In fine, whatever produces on the stomach a very disagreeable, more especially a nauseating impression, diminishes the force and generally the frequency of the contractions of the heart. This it does sympathetically, by deranging the condition of the brain and nerves. A certain amount of nervous influence is necessary to the healthy and vigorous action of the heart. Diminish that amount, and the action is enfeebled. Hence whatever produces such diminution, although it be by stimulation, is fallaciously denominated a sedative. The reason of this is obvious. The action of the heart is mistakingly made the only test of sedatives and stimulants. Whatever can be made to diminish that is classed with the former.

According to this rule, I say, every thing can be proved to be a sedative, that can be made to produce exhaustion, by means of excessive stimulation. And every powerful stimulus does that. Arsenic, corrosive sublimate, and sulphate of copper can be used to this effect. So can most other potent poisons; and the time is coming when even the prussic acid will be employed in the same way. By the mode of reasoning here employed, the "counter-stimulants" of the Italian and other physicians, about which so much has been said and written, can be clearly proved to be all irritants.

Agreeably to the rule by which I am examining this subject, a severe blow on the region of the stomach or the head, is as genuine a sedative, as digitalis, cold, or opium. By the shock it produces on the cerebral and nervous systems, it so deranges their functions, and impairs their energies, as to weaken the pulse, and so detract from the strength and tone of the muscles

and the body generally, that the individual injured often drops to the ground in a state of insensibility. It is on the same principles that fatal burns, and some of the most powerful poisons seem to act. They so shock, by their impression, the nerves and brain, as to destroy their functions; and hence, for want of the sustaining influence of those organs, the system is prostrated, and the sufferer expires.

Hunger is called a sedative sensation, and fear, grief, and despair sedative passions. Names more directly opposed to fact, and, therefore, more inappropriate and deceptive, have never been bestowed. As well may a gun-shot wound, a cut with a sabre, or the fracture of a bone be denominated a sedative. True, all those agents may so operate as to reduce the force and frequency of the pulse. But they do it by exhausting the energies of the system, in consequence of a previous overwhelming impression on the nerves and brain. They wear out the excitability of the system, and produce in it what may be denominated a state of temporary collapse. On what principles this collapse is followed by a state of exacerbated action, I shall not now inquire. The fact is all that is material to me at present.

By many pathological writers the matter productive of fevers generally is pronounced a sedative. Why? Because it enfeebles the action of the heart. Previously to the stage of excitement, say they, the pulse is always small and weak—granted; but is this the first link in the chain of fever? No pathologist will reply affirmatively. The febrile poison does not act primitively on the heart. It reaches that organ secondarily, throwing its influence on it through the medium of sympathy. The poison produces, therefore, some previous affection.

The heart and arteries are not the seat of fever. Their deranged action, whether too high or too low, is purely sympathetic. It is as really a symptom as chilliness, an aching of the limbs, or a pain in the head.

The genuine seat of fever, where it first takes root, is the stomach, and some of the other abdominal viscera. To the truth of this, symptoms before death, and dissection afterwards,

abundantly testify. That I may speak more definitely, let the subject considered be bilious fever.

Of the poison of that disease the primitive action is obviously on the stomach. But is its impression on that organ sedative? Most palpably it is not, as every phenomenon connected with it demonstrates. It is irritative, as the congestion it produces first, and the morbid sensations afterwards, conclusively testify. This deleterious impression on an organ so important and commanding, deranges seriously the nerves and brain. In consequence of this, the sustaining influence of those tissues being withheld from the heart, that organ fails in the vigor of its action; and the blood, which it is no longer able to propel with competent force, accumulates within it, and in the vessels around it. Hence congestion in the large blood-vessels is by no means a primary phenomenon of fever. It is but a single link in the chain of the disease, several essential links having preceded it, and contributed to produce it.

In the production of bilious fever, the following would seem to be the order and progress of action. The febrile poison, whatever it may be, *irritates* the stomach, giving rise to congestion always, and frequently to inflammation. By sympathy the nerves and brain suffer. Their influence being withheld from the heart, muscles, and skin, those organs suffer next in course. As yet the succession of phenomena has been deleterious. But now come those that are salutary. These are the hot stage, and the sweating or rather secreting stage, the latter of which is intended by nature to terminate the paroxysm; and it usually does so. The poison of bilious fever, then, is a stimulant. It excites and irritates the stomach, and acts, by sympathy, as a sedative on the heart and other organs.

Of the true nature, and primitive action, of the poison of small pox we are still more perfectly informed. We know it, by actual inspection, to be an irritant. Insert it in the skin, by the puncture of a lancet, and it irritates and visibly inflames the part which it touches. But in the progress of its career it reaches the heart by sympathy, and enfeebles its action, precisely like the poison of bilious fever.

Of every febrile poison the same is true. It begins its attack on an outpost of the system. To that organ it is an irritant. To the heart it proves a sedative, by depriving it of much of the nervous influence. On this point I shall only add, that to me the notion of the sedative nature of a genuine febrile poison, appears one of the most incongruous in the science of pathology.

In further illustration of this topic, let us contemplate, for a moment, the phenomena of fever produced by mechanical injury. Let the injury be a severe burn, a cut with a sabre, or a gun-shot wound. Previously to the stage of excitement, there is here, as in every other form of fever, a perceptible diminution of both the force and volume of the pulse. The heart, therefore, is palpably enfeebled in its powers of action. But who will hence infer, that a burn, or a wound from a sabre or fire-arms is, in its nature, a sedative? No one surely who has the slightest knowledge of the principles and laws of living matter, or the relation it bears to surrounding nature. But as well may a mechanical injury of any description be termed a sedative, as the poison productive of bilious fever.

The more fully to confirm what I have said of the nature and character of mechanical injuries, and of febrile poisons, it is important to observe, that it is not until sometime after the former has been received and the latter applied, that they enfeeble the action of the heart, preparatory to the augmented excitement which follows. Yet during this time they have not been inactive. They have been producing on the brain and nerves that effect, from which, operating in its turn as a cause, arises the debility of the heart and muscles of voluntary motion.

Such are some of the principles on which the human system is constructed and endowed, and some of the laws by which it is governed. To me they appear to shed satisfactory light on the subject of sedatives, and to prove definitely that the doctrine is erroneous.

ART. IV. *Some account of the Lunatic Asylum of Kentucky, with Remarks, &c.* By SAMUEL THEOBALD M. D. attending Physician of the Institution.

EVERY institution having for its object the relief or alleviation of the afflictions of mankind, justly claims the attention of the community. The great aim of all human exertion is the avoidance of suffering and the procurement of comfort and happiness. And that individual or community by whose wisdom these great ends are most extensively and certainly promoted, holds the highest claims on the gratitude and benediction of our race. In every country there are to be found numerous individuals who are entirely disqualified from securing their own happiness or enjoyment; and not a few who are destitute of friends or of patrimony to contribute to their support. Hence in every well regulated government, we find charitable provisions, both public and private, for the benefit of such individuals.

It is acknowledged by all, that of the numerous ills to which mankind are liable, none can compare with mental alienation. We shudder and shrink at the idea of dissolution, as of all things most appalling, and yet where is the individual who would not unhesitatingly welcome the king of terrors, rather than become the victim of confirmed madness. If we regard then the magnitude of the evil to be arrested, as the criterion in our estimate of the value and importance of humane and charitable institutions, none present higher claims to our consideration than those designed for the care and relief of the insane.

Entertaining no doubt but that a more general diffusion of information, in relation to the important advantages resulting from the establishment of institutions of this character, would tend greatly to promote the cause of humanity; and at the same time prove no contemptible means of economising the public revenue appropriated to the support of this unfortunate class of

our fellow beings; I have thought proper to communicate through this Journal some account of the Lunatic Asylum of our State.

It is not my purpose at present to offer any remarks, except in very general terms, as to the medical or moral treatment of the patients of this institution; but only to present a statement of some of the leading facts connected with its establishment and present condition: such as would more immediately interest those to know, who might be inclined to contribute towards the founding of similar institutions.

The Lunatic Asylum of Kentucky was established at this place by an act of the Legislature passed on the 7th of December, 1822. That act premised that "the consideration of public safety, the well being of society and long experience, enforced by the example of other countries conspire to prove the necessity of providing by law for the care, comfort and safe keeping of persons mentally diseased, and who are of unsound minds. And a full and fair trial having proved that the present laws of this state are highly defective and are not calculated to aid in the best manner the restoration and final cure of such persons; and yet are found to be enormously expensive in their operation, and rapidly increasing: For remedy whereof, &c."

The first section of the act provides for the establishment of a Lunatic Asylum in the county of Fayette, near the town of Lexington; and that Richard Higgins, Elisha Warfield and John W. Hunt, of the county of Fayette, John Pope, of the county of Washington, and Wm. P. Roper of the county of Fleming, be appointed Commissioners, to purchase, on behalf of the Commonwealth, a suitable lot of ground, including a spring of never failing water, of not less than 10, nor more than 20 acres, for the public purpose aforesaid. The Commissioners were required to proceed immediately to make the purchase, upon the best terms practicable, and proceed to erect, or cause to be erected suitable and convenient buildings of stone or brick, or both, sufficient for the care and accommodation of at least 200 persons; having due regard to their comfort as well as safe keeping. They were further required to make use of all reasonable despatch in erecting and completing the building, and in providing the ne-

cessary maintainance, with plain, cheap lodging for the proposed number of lunatics; and when provided, to report the same to the Executive of the State, who was required to make proclamation thereof, giving notice to all committees of lunatics, and to the circuit courts, that the Asylum was ready for the reception of its subjects. From and after a day to be named in the proclamation, (allowing reasonable time) the whole of the laws then in force in this State in relation to the maintenance of insane persons were to cease.

The Commissioners were further authorized, in making the purchase, to select a lot or tract of land with suitable buildings thereon, or partly erected, having due regard to the provisions of the act.

It was made the duty of the Governor to appoint some suitable person as Steward so soon as it might be necessary, to hold his office until the close of the next General Assembly thereafter, and to receive at the rate of $500 per annum for his services.

The sum of $10,000 was appropriated, to be drawn quarterly, for the purpose of carrying the act into effect, &c.

On the 11th December, during the same session, a supplemental act was passed, which provided, that if it should be made appear to the satisfaction of the Circuit Court, before which any person hath been or shall be found a lunatic or idiot, that said lunatic or idiot, is harmless and can be safely and properly kept by his or her parent or near relation, and that said lunatic or idiot hath not sufficient property to support him or her, the said Court may in its discretion appoint a committee and make an allowance as heretofore, &c.

On the 7th January, 1824, an Act passed to carry into operation the Lunatic Asylum; and the following gentlemen were appointed Commissioners for that purpose, viz. John W. Hunt, Richard Higgins, Elisha Warfield, John Brand, Wm. W. Worsley, Richard H. Chinn, William Morton, John Bradford, Andrew McCalla, and John R. Witherspoon; with authority to purchase, or cause it to be done, all necessary articles for the institution, and to hold their appointments for two years or until

others should be appointed by law. They were required carefully and scrupulously to examine the case of every subject brought to the Asylum, distinguishing by all the means in their power, between such persons as might be sick or imbecile only, and such as were actually lunatic or of unsound mind; admitting only the latter. Also carefully to distinguish maniacs, or persons who are dangerous, from such as are quiet and peaceable, making orders for their confinement or otherwise.

The first day of May, ensuing the passage of the act, was fixed as the period at which all laws committing persons of unsound mind to the care of committees, and charging the treasury of the State therewith, should cease; and that thereafter the care and safe-keeping of all such persons should be confided to the Lunatic Asylum. Upon the production of any lunatic or other person of unsound mind at the Asylum, and examination had, if the person offered aforesaid should be found to be a fit subject for admission, he or she was to be provided for according to his or her state of disease; and the reasonable expenses of conveying the patient, to be paid to the person delivering him, her or them, by the Commissioners, or by their order. The Commissioners were further invested with full power and authority to discharge any person or persons from the Asylum, whenever they deemed such person or persons sufficiently restored. They were required to cause an account to be kept of all their transactions and disbursements, to be laid before the Legislature annually. They were authorized to draw from the Treasury the sum of $10,000, to be applied to the purposes mentioned in the act. Also, to make bye-laws for the government of the Asylum, not inconsistent with the laws of the state. By this act also, the authority of appointing the Steward or keeper was conferred on the Commissioners, and so much of the former act repealed as conferred that power on the Governor. The 9th section of this act provides that idiots shall be permitted to remain with their parent or parents, if they so desire, and that such parent or parents shall be authorized to continue in possession of such idiot as heretofore directed by law: provided, however, that the Circuit Court shall not make a greater allowance for the keeping of

such idiot than it would cost for keeping such person in the Asylum. By the 10th section, it is further enacted that nothing contained in the act, should be so construed as to authorize the managers to support any persons in the Asylum, at the public charge, who have estate sufficient for their support; and that in all cases where an inquisition is found, the same shall accompany the individual sent to the Asylum.

On the 10th of January, 1825, an act passed further to regulate the Lunatic Asylum, by which it was provided, that vacancies occurring in the Board of Commissioners should not be filled until the number was reduced below five: also, conferring on them the power to make such alterations in the Asylum, and to erect such stoves, walls, cells, &c. as were absolutely necessary to the safe keeping and comfort of those confined therein; and appropriating the sum of $3,000 to effect these objects. By the same act the further sum of $7,000 was appropriated for the maintenance of the lunatics. It also provides that the sum allowed for the support of idiots, who are kept by their parents, shall not exceed $50 per annum; it having been ascertained that the cost of keeping them in the Asylum does not exceed that sum.

At a subsequent session on the 21st December, 1825, an act was passed making provision for the keeping of lunatics, in certain cases; by which it was provided, that individuals who had kept lunatics or idiots, who had no estate, between the 1st day of May, 1824, (the period at which the Asylum was opened) and the 1st day of December, 1825, and no longer, should be allowed compensation therefor at the rate of $50 per annum.

On the 24th of January, 1827, an act passed concerning the Lunatic Asylum, the object of which was to remunerate certain individuals who had expended large sums towards the purchase of the tract of land, and the erection of the original building of the Asylum; in the purchase of which the state had hitherto only paid the sum of $600, which was a balance due for the land, from those individuals. By this act the sum of $6,000 was made subject to the payment of all properly authenticated claims, by those who had expended money, labour or materials in the erection of the building, &c.

By an act for the appropriation of money, passed on the 25th of January, 1827, the sum of $6,000 was appropriated for the support of the lunatics in the Asylum.

By an act of the 9th February, 1828, the sum of $6,000 was appropriated for the support of the institution for that year. It provided also that paupers who are relieved and discharged, should be allowed a reasonable sum, out of the funds of the institution, to defray their expenses home. It further provides for the taking receipts, keeping accounts, &c. in all purchases made for the Asylum; to be submitted to the Legislature, or any Committee thereof when required. The Commissioners were also required to ascertain and report the terms on which the ground could be enclosed, &c. Whether any additional ground could be purchased, &c. It was likewise made the duty of the Commissioners, (a majority of them concurring therein,) to draw up a code of rules for the government of the Institution, which should prescribe the duties of the several officers and assistants.

An act passed the 29th of January, 1829, appropriating the sum of $6,200 for the support of the lunatics of the Asylum for the present year. Also, the sum of $1,200 to erect three permanent walls, to enclose a yard around the Asylum for the exercise of the patients. Also, the sum of $3,500 to erect an additional wing thereto to contain 15 or 20 rooms.

We have here presented the humane and politic motives which influenced the Legislature of Kentucky, in the establishment of this Institution; together with the substance of all its acts in relation to it up to the present period.

The Commissioners appointed by the act establishing the Institution, proceeded with the utmost promptitude to the discharge of the duties devolving upon them; and selected as presenting the highest advantages, a tract of land containing about 17 acres, and having thereon a spring of never failing water, as also a large and handsome brick edifice, which had been constructed about the year 1817, by an association of individuals, as a private hospital for the diseased of every character: but the Company having failed in their humane intentions, the building

remained unfinished and unoccupied. Having purchased this property they proceeded immediately to have the building finished, and such additional improvements made as were then deemed adequate to the object in view; and on the 1st day of May, 1824, the house was prepared and opened for the reception of patients.

It is situated on the North-west of the town, and 2 or 300 yards within the mile square which determines its limits, yet sufficiently without the business and bustle of the streets to be entirely free from any annoyance or inconvenience from that source. The buildings of the Institution are all of the best brick work, on stone bases, extensive and handsomely constructed. They consist, at present, of a centre or main building about 66 feet square, four stories in height, including the basement, and containing 25 rooms, one half of them spacious, and adapted to the purposes of accommodating the family of the Superintendant, the resident Physician, and separate day-rooms and refectories for the male and female patients; as also one for surgical purposes on the upper floor, handsomely lighted from the roof. This is the original building before alluded to. There have been constructed since, two wings, each three stories in height, including the basement, 62 feet in length by 24 in width, containing 16 rooms each—6 on each floor above the basement, with spacious aisles running the whole length of the wings in each story, and meeting corresponding aisles in the main building. During the present season a very handsome and extensive addition has been made to the North-west wing, in the form of an L, which completes the symmetry of the building in that direction. This is of a corresponding length and width with the other wings, but is four stories in height including the basement, and contains 32 rooms. Besides these, there is, in the rear of the centre edifice, removed about forty paces distant, a building 20 feet square, two stories high, and containing 16 small rooms or cells, intended for the worst class of patients, and for withdrawing temporarily such as are most turbulent and ungovernable.

All the rooms except in the centre building are supplied with heat through flues, passing from the basement story so as to

avoid entirely any danger from fire in this portion of the buildings. Except in very cold weather, the heat thus supplied is adequate to render the rooms entirely comfortable. Some improvement however in this respect is called for. Whether any other plan now in use than the one adopted here, is better suited to answer this purpose I am not prepared to say. I will remark here that this is a subject of importance in every institution of this character, and demands special regard; scarcely so much with a view to avoid danger, and consult economy, as to supply a regular and comfortable degree of heat, in cold weather, to those wretched beings, many of whom at particular periods, and not a few at all times, are entirely regardless of the ordinary means of rendering themselves comfortable, either by attention to their dress or bed clothing, so that they will often remain exposed, without covering, in the severest weather.

A permanent wall of brick to enclose about an acre and a half, in the rear of the buildings, has been commenced, and will likewise be completed in a short time. The enclosures heretofore have consisted of wood, and although made as secure as could well be done, they have not been sufficient entirely to prevent escapes. This is a subject which has engaged the frequent and earnest consideration of the managers, and they have not been wanting in efforts to remedy the inconvenience. The late act of the Legislature providing for the erection of a permanent wall, as just mentioned, will certainly be a valuable improvement, and, to a considerable extent, obviate the deficiencies heretofore existing. It is yet obvious to every one that great advantages would result from further attention to this subject. The ground to be enclosed by the wall now going forward, is intended to be, and is now in fact divided, but in part by plank enclosures, into three yards; one for females and the other two for males; so as to allow a separation of the latter into two classes.

The whole extent of land at present owned by the Institution is about 17 acres. Of this near 4 acres are appropriated as a yard in front of the Asylum. This, handsomely set with grass, and ornamented with trees, stretches a distance of about 150 yards to the street or road, along which is the only approach to

the buildings. About mid-way between the road and house is a spring of abundant, never failing, and excellent water, from which the Institution is entirely supplied. Here too is a dairy, wash house, and bathing house under the same roof. Another portion of the ground, embracing about 5 acres, is appropriated as a garden, in which a variety of vegetables is cultivated, and in sufficient quantity to supply the demand of the institution for great portion of the year. The residue is appropriated for the out buildings, stable, cow-house, &c. These are enclosed at present by a common post and rail fence.

It is important in all cases where the situation of the patient will allow it, that he or she should be regularly employed in the domestic occupations of the house and grounds. It is also frequently very desirable, not only as an agreeable indulgence, but as a means of restoration, to allow individuals the privilege of more pleasant and extensive walks than can be enjoyed in the closed yards; and all this without the necessity of incessant watching on the part of the keepers, although this never should be remitted further than becomes indispensably necessary. To attain these important advantages in their fullest extent, it is indispensable that the whole grounds, or to the limits of the front yard and garden at least, should be so enclosed as to obviate the great risk of escapes that exists at present; and this might be effected, to a reasonable extent at least, by high and well constructed plank fencing.

The location of the Institution is certainly the most judicious that could have been selected by the Legislature. It may be safely asserted, that in point of salubrity none can surpass it. The surrounding country to a great extent is naturally as fertile and beautiful as the world has ever presented; and the hand of art has been liberally bestowed, to render these advantages most subservient to the wants and comforts of its inhabitants. The proximity of the Asylum to our market ensures a regular and abundant supply of all the necessaries and luxuries of our land, without inconvenience, and on the most moderate terms. Whilst the extent and character of the population of the town, and neighbourhood, furnishes an ample field for the selection of in-

dividuals highly qualified to manage its concerns, both in a civil and medical point of view.

The particular site of the buildings could not probably have been more happily, or advantageously fixed. It is sufficiently elevated and withdrawn from other improvements to combine the advantages of the freest circulation of air, and a ready discharge of all the surplus water that falls, with the handsomest display of the buildings. As has been previously noticed, it is near an inexhaustible fountain of pure and excellent water, and although near enough to enjoy all the conveniences and advantages of the town, yet sufficiently retired to be without any interruption from its noise and bustle.

There have been admitted to this Institution since its opening in May, 1824, and up to 15th of September of the present year, 214 insane persons. Of this number 137 were males and 77 females. There have occurred in the same period the death of 34 males, and 13 females; in the cases of males one from suicide, and one by accident. There have been discharged cured 35 males, and 8 females. Also, 6 males taken out by their friends much relieved, and 9 males and 7 females stationary. 7 males and 4 females have eloped—leaving in the Institution on the 15th September, 1829, 91 individvals, viz. 47 males and 44 females, of the whole number admitted only a very small proportion were boarders.

There have occured cases of Idiocy, : : :	30
Convulsions, (existing at the period of admission from 3 to 20 years,)	22
Insanity from other causes, existing 1 year and not over 2	27
Over 2 and not over 5, : : : : :	49
" 5 " " 10, : : : : :	19
" 10 : : : : : : : :	35

Previous to the establishment of this Institution, (as has already appeared from the recital of the foregoing acts) the state of Kentucky, like most of her sister states, had pursued the system of providing for the care of the insane paupers, by the appointment of one or more individuals to take charge of them, who

were allowed, at the discretion of the judge before whom the case was presented, such sum as was deemed reasonable for the trouble aud expense of taking care of them. The defect of such a system, although perhaps the best that can readily be adopted in the infancy of governments, is obvious and striking upon the slightest reflection. Under such a system, the care of these unfortunate beings, in a large majority of instances, will be committed to individuals uninformed and often inhumane, who have no other motive to take the requisite, and proper care of them, than the pecuniary advantage which they expect to derive: hence they will be careless of their comfort, and negligent in restraining them from wandering abroad. This the insane are very generally prone to do, and even where every reasonable and particular care is observed on the part of those who have charge of them, to avoid this inconvenience and evil, it cannot always be done, for want of knowledge as to the best mode of governing them, but more especially for want of the requisite and proper physical means of restraining and controlling them. Who has not observed in every community, where this system is pursued, the manifest evil resulting from this single circumstance. How often have we seen the wretched maniac at large in society, wandering from house to house, and from village to village, exposed night and day to the inclemencies of the weather, without shelter, and without food, or ordinary raiment; often excited to paroxysms of rage and fury by the conversation or acts of the idle and unthinking. Not unfrequently indulged too, by the thoughtless or vicious, in the use of ardent spirits, for which they have generally a strong propensity. Thus rendering them a terror to society, and confirming their most miserable condition, not only by the entire neglect of any means of restoration, but by superadding the baneful influence of violent and uncontrolled passion, often aroused; and the excitement of artificial stimuli.

But the disadvantage resulting to this unfortunate portion of society under such a system, although justly demanding the first consideration, is not all. The pecuniary interest of the state, as most strikingly appears in the experience of Kentucky, is materially involved. It appears by a recurrence to facts on this

subject that in the year ending 10th of October, 1822, the state of Kentucky paid for the support of insane persons, the sum of $15,490. 44. And that for the year next ensuing, the Auditor of public accounts, in his report to the Legislature, stated that the appropriation would require to be increased to 18,000. Since the establishment of this Institution, this item of expenditure has been curtailed in a most extraordinary degree, as may be observed from the acts of appropriation. The expenses of the Institution, other than those incurred in the construction of permanent improvements, have not much exceeded one third the former amount. To present this matter in a still more striking point of view, we may remark, that the Institution has now been in operation a little upwards of five years. Taking the sum last reported by the Auditor, as requisite for the support of lunatics, for the standing appropriation of each year, viz. $18,000, (which indeed is less than ought to be calculated, because this item of expenditure had regularly and largely increased every year) and in the period just mentioned, it will amount to the sum of $90,000. Now it may be seen from what has been previously stated that the entire appropriations towards the Asylum, only amount to $48,900, which embraces not only the sums annually applied to the support of the insane, but the whole amount expended for the extensive and valuable property, heretofore described, appropriated to their accommodation.

[TO BE CONTINUED.]

ART. V. *On the Cultivation of certain Medicinal Plants.* By CHARLES W. SHORT, M. D. &c.

THE deterioration of many vegetable medicines, by long keeping, exposure to light and air, the depredations of insects and the injury of moisture; together with their adulteration by admixture with inert or injurious articles, are notorious to all who are conversant with them, and a frequent cause of regret and disappointment, in their practical applications to the cure of disease. As respects many of these medicines, for our supplies of which we are compelled to rely on foreign importation, the difficulties alluded to are only to be remedied by a careful selection of them, from druggists of approved standing and knowledge in their business; and afterwards by a diligent preservation and seclusion of them from the access of all injurious agents. These methods of protection are in some degree peculiar to each article, and need not here be pointed out. In every instance, however, the physician who has a proper respect for his own character, or regard for the safety of his patients, will promptly reject from his stock any substance whose excellence may even be suspected, and supply its place with the best of the kind which he can possibly obtain. For surely the most inexcusable of all kinds of parsimony is that, which would influence a practitioner to prefer an inferior article to one of decided excellence, merely on account of a few dollars difference in the price. In every well regulated drug-store or private shop the supplies of all drugs of a perishable nature should be frequently renewed, although no positive evidence of their deterioration may exist; for it is certainly erroneous to suppose, that any dead vegetable substance can improve, either in flavour or effect, by long keeping. As soon as articles of this kind are, by exsiccation sufficiently deprived of their sap and other vegetable juices to prevent decomposition, they have attained all the per-

fection of which, in that state, they are susceptible; and perhaps every day afterwards detracts in some degree from their virtues. The truth of this principle is constantly recognized in the common concerns of life, by the selection of the newest hay for the stall, and the freshest tea for the table. Why should less attention be paid to those more important, but equally perishable agents, upon which we venture the lives of human beings?

Fortunately, however, as regards many important articles of the vegetable materia medica, it is in the power of most physicians, and particularly of those who reside in the interior of country situations, where opportunities of resorting to good drug-stores are rare, of commanding a supply of these medicines, upon which they may at all times rely with confident assurances of their freshness and purity—by their own cultivation of them.

With the view of drawing to this subject the attention of the profession, and especially those members of it whose time and opportunity enable them to undertake it, the present paper is written. The objects it proposes, are not matters of conjecture; they have been practically tested by the writer, and their feasibility satisfactorily established: he, therefore, confidently recommends them to his professional brethren. To such of them as are engaged in farming, or are fond of horticultural pursuits, an attention to the culture of the plants alluded to, independently of their intrinsic utilities, will be found a source of pleasurable relaxation, from severer duties.

Digitalis purpurea. Purple Foxglove.

This powerful medicine has had its day of reputation, followed but too soon, we are convinced, by the night of neglect. That it is possessed of most active properties, and is capable of being made to produce signal impressions on the human system, none are prepared to deny; and although its virtues are still highly extolled by some, as exercising a most controlling agency over diseased action, its use is vehemently denounced by others, of high standing in the profession, as always uncertain, and frequently unsafe. Perhaps these discrepant testimonies, and con-

flicting opinions, in relation to its employment, are in some degree to be accounted for, from the circumstances already alluded to—its deterioration by age, and other noxious agents. As frequently met with in the shops of western practitioners, the Digitalis is found to be a mass of faded worm-eaten leaves, which have, perhaps, for years been exposed, in open jars or drawers, to the injurious influence of air, light, moisture, insects, &c. and from it, in such a state, very different and inferior effects are to be expected, to those of the recent leaf, when properly prepared. It is, however, in the power of every physician to use this medicine in its purity, since the plant is one of the easiest cultivation in all parts of the Union.

In the different countries of Europe, to which it is indigenous, the Foxglove prefers dry, chalky, loamy or gravelly soils; indeed, is it found in all except such as are too damp, or marshy. In cultivating it, in this country, the situation chosen should be dry and not too much shaded, nor exposed too directly to the power of the sun. The seed may be put into the ground towards the last of April; but being very minute they are apt to be sown too thick; attention is consequently necessary to thinning the plants, when they have attained any size; care being taken not to leave them nearer than two feet apart. The Foxglove being a biennial plant, does not arrive at perfection, or flower until the second summer; but in this latitude it requires no protection during the intervening winter, and even in the severest weather the leaves retain some degree of verdure. About June of the second year, it sends up from a dense tuft of large root-leaves, a long, graceful spike of beautiful, purple flowers, elegantly dotted in the interior with eye-like spots, which continue unfolding for a number of weeks, and are succeeded by small capsules filled with minute seed. The largest and first of these seed-vessels should be reserved for further culture, securing them as soon as they turn brown and begin to open.

The directions given by Dr. Withering, (who has paid more attention to the preparation and exhibition of this article than any other physician,) for the collection and preservation of the plant are, that, "the leaves be gathered when the flower-stems

are shot up, but before the time of flowering; and the largest and deepest coloured leaves are to be preferred. These are to be carefully dried in a warm room through which a current of air is passing; and when perfectly crisp and dry, they are to be reduced to powder, and kept in bottles closely corked, and not exposed to the light. In this state the digitalis has a fragrant smell, not unlike new hay. The dried plant, well kept, will preserve its virtues for a considerable time, but as uniformity in strength is always desirable; and as all herbaceous plants gradually lose their medicinal properties, it will be right for the apothecary to prepare it afresh every year."*

A variety of this species is sometimes met with, having white flowers, which is less esteemed for medical use than the purple flowered. A dozen thrifty plants of the latter, raised on the borders of a garden, where they are highly ornamental, will afford leaves enough for the supply of any physician of the most extensive practice.

Cassia Senna. *Senna.*

The seeds of this most valuable cathartic plant were probably first introduced into the U. States by imported parcels of the leaves, in which ripe capsules are occasionally found: and the plant has consequently been cultivated for several years in different parts of the country.

In a number of the *Raleigh Star* printed in the Spring of 1813, and republished in the *Eclectic Repertory* for January, 1816, is a short paper on the cultivation of this article; by which it appears that, some years previously, the Medical Society of North Carolina had offered a premium for the production of the largest quantity of Senna, exceeding forty pounds, raised by one person in one year; and that in consequence thereof the writer cultivated one quarter of an acre in the plant, and obtained therefrom between fifty and sixty pounds of Senna, which physicians said was much superior to that imported from the coast of Barbary, and equal to the best from the Levant As this is the only article, of which I am apprized,

* Rees' Cyclopedia, article, Digitalis.

that has been published in this country, on the culture of this plant; and as the directions given by the writer, (whose name is not known to me,) have been succesfully followed by myself, in recent experiments, I will give the substance of them in his own words, adding some remarks on particulars in which my own observations do not agree with his.

"It is a delicate plant, sensible to cold, rains and drought: the seeds should be committed to the earth about the first of May; the ground should be mellowed and the seeds planted in rows, covered an inch deep and four inches asunder. The rows should be a foot or two apart, so as to admit of being worked, and the only tillage necessary is to keep it clear of weeds and to earth it moderately. If the season is dry or the soil thirsty, it would be benefitted by an occasional watering. The plant in good soils grows to two-thirds of the height of indigo, but spreads more. The leaves (the valuable part) grow on long stems issuing from the trunk and branches.

"Besides a drought, the Senna is exposed to another formidable enemy. When in bloom it is visited by a yellow butterfly which deposites its eggs upon the leaves in vast abundance. The eggs in a few days become worms, which destroy the leaves. I know no means of preventing the visitation of these troublesome flies, or any expeditious mode of staying the advances of these eggs to the animal state. Sprinkling ashes over the shrubs has been recommended by some, and has suggested itself to me, as a probable means of being useful in both cases, but I have never tried it. The only means which I have attempted was to brush off the eggs with a wing where I have detected them. This is a work of some labour. The eggs are small and white, and will not be observed without minute inspection. The later in the season the plant is in coming to maturity the more liable it is to the depredations of this butterfly progeny. Hence the propriety of bringing it forward as early as the season and its tender nature will permit.

"The Senna is to be gathered once or twice a week as the plant is going on to maturity. When the lower leaves have gained their full size, and begin to change their colour a little,

pluck off a few courses of the lower stems; wilt them two or three hours in the sun, then spread and dry them in the shade, pick out the stems, and put up the leaves in bags suspended in an airy dry situation.

"The seeds are contained in pods which grow at the extremity of the stalk and its branches. As the ripen they become black and fall off. Gathering the leaves does not at all injure the production of seed.

"A light rich soil is best. If sandy, it is liable to suffer from drought."

My own experience would so far differ from the above as to suggest the sowing of the seed at an earlier period, even in this latitude; for they are slow in vegetating, at whatever season they be sown, (sometimes, indeed, requiring four and six weeks for this process,) and unless the plants are urged forward as early in the Spring as possible, they do not yield an abundant harvest of leaves, before they are killed by the frost.

In the course of three or four years experience with it, I have never once seen the butterfly alluded to in the communication above, nor ever noticed the slightest injury to the plant from any insect whatever; a circumstance from which we may infer that this latitude is even more favourable to the growth of the Senna, than that of the neighbourhood of Raleigh.

I have observed it moreover, to flourish best in a rich soil, having a small admixture of sand, and that the product of leaves was the greatest in warm, dry summers.

If, as would appear from the observations of Mons. Hippolite Nectoux,* that even the best Alexandrian Senna, as exported from Egypt, consists not only of the leaves of different species of Cassia, viz. *Cassia Senna* and *C. Lanceolata*, but even of other plants of different genera, it is surely desirable, with a view of avoiding these adulterations and admixtures, that the true Senna be more extensively introduced and cultivated among us. Whatever may be the relative virtues of the different components, which go to make up our parcels of imported Senna,

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* Barton's Vegetable Materia Medica of the United States, vol. i. p. 143.

there can be no question but that the plant, to which in this paper we particularly invite the attention of the profession, and which is the true *Cassia Senna* of Linnæus, is every way worthy of their attention, as a cathartic medicine; to our indigenous Senna, (*Cassia Marilandica*,) it is in every respect superior.

Ricinus communis. Castor-oil plant.

Although the oil obtained from the seeds of this plant, in consequence of its great cheapness, is but little subject to sophistication, and is almost always to be had in a state of great purity and freshness, yet as it is a medicine of very general use, and such deserved reputation, both in the hands of the physician, and in domestic practice; and as it is, moreover, becoming an article of considerable traffic in many parts of the country, a few words on the subject of its culture and manipulation, derived from experience in them, may not be amiss in this place.

This plant, commonly called *Palma Christi*, and so frequently raised in country gardens, under the idea of its protecting other vegetables from the ravages of the mole, is subject to considerable varieties. Three sorts are most generally met with;—one having a deep purple stock, and leaves tinged with the same colour, growing luxuriantly and bearing large purple seed, of which, however, in this latitude it does not mature a great many. A second variety has a faint blush of purple on the stalk, but the leaves are entirely green; it does not grow so luxuriantly high as the preceding, but matures a greater number of seeds, which are smaller and of a less deep colour. The third kind is of still humbler, more diffuse and branching growth; it is entirely of a bluish green, and bears a great profusion of seed; these however are smaller than either of the others, consequently more troublesome to gather, and less profitable in the outturn. From these circumstances the second mentioned variety has been uniformly prefered for cultivation; and an opinion has even been entertained that the first kind yields an oil much more acrid than either of the others, but with what truth I am unable to determine.

The cultivation of the Palma Christi is of the easiest nature,

being very similar to that of corn. A piece of ground being selected (which should be light and loamy, but not of the richest quality,) it should be deeply ploughed in the fall, and subjected, undisturbed throughout the winter, to the influence of frost. As soon in the Spring as it is dry enough, and the earlier the better, it should be again ploughed, harrowed and laid off by furrows five or six feet apart, crossing each other at right angles. Three or four seeds are then dropped at each intersection of the furrows, and lightly covered. When they have come up, and the danger of injury from the frost is over, they must be thinned, leaving one plant only in each spot. Whilst small they may be cultivated with the horse and plough, but when attaining any considerable size, they branch out laterally to such a degree that the hoe alone can be used. Throughout the whole of the season, from the first planting until the last gathering, it is necessary to keep them clear of weeds and well cultivated. When the seeds begin to ripen, which is shown by the seed-vessels becoming dry, and discharging their contents elastically, the gathering of the crop should immediately commence. This is best done by passing along the rows with large baskets and cutting off, with a strong knife, those bunches of seed which are beginning to open; and this operation must be repeated daily, (in the morning or evening,) until the whole crop is collected; for successive flowers are formed, and seeds ripened from mid-summer, until farther vegetation is arrested by the frost.

The separation of the seed from the husk or vessels is the next step in the process, and this is attended with no little trouble; for the seeds, being exceedingly brittle, cannot be threshed, and if not properly guarded they are lost; being thrown to a great distance by the sudden bursting and contraction of the capsules. To prevent this they should be laid, as soon as they are gathered, upon a tight floor, surrounded by close walls, admitting, in dry weather, all the air and sun-shine possible. A preferable plan is to select a garret room, not lathed or plaistered, but having a close floor, and several windows. In this room a number of light scaffolds are to be erected, one above another, and covered with lathes at a few inches distance. Upon

these frames the bunches are laid as they are gathered; the heat and air of the situation favour their drying, and when the seeds pop out they are secured. When this process is over, the seeds are separated from the husks by sieves of a proper size, and are then ready for the manufacturer.

Formerly, as is well known, these seeds were mashed in large mortars, and the pulp subjected to the action of boiling water, by which means the oil was separated and skimmed off. The oil thus obtained has a deep amber colour, is unpleasant to the taste, and disposed to rancidity. Of late, however, its extraction by pressure in a mill, in the same manner with the lintseed-oil, has become general in this country; and the oil thus obtained is known as the cold-drawn or cold-expressed Castor-oil. When first extracted it is turbid or muddy, but by processes, known only to the manufacturer, it is rendered perfectly transparent and colourless, with little or no taste or smell. It may, however, be a matter of question whether the modern improvements, in the purifying and clarification of this oil, be not really a disadvantage;—a sacrifice of practical utility to fastidious taste; for every practitioner who has made use of both, must have observed the difference in their action; that while the pale oil is sometimes scarcely laxative, the red is generally an active purgative. So entirely, however, has the former superceded the latter, that a bottle of the red oil is now scarcely ever to be met with.

An acre of ground, well cultivated, will ordinarily yield about 20 bushels of clean seed, each bushel of which will afford a gallon and a half of clear oil.

Papaver Somniferum. Poppy.

All this extensive tribe of plants, of which twelve or fifteen distinct species are enumerated by botanists, together with their innumerable varieties, yield, when their capsules or heads are wounded, a milky juice, which on concreting becomes first brown and then black, and forms pure opium without farther preparation. Nor does the article, as obtained from any of these species or varieties, differ essentially in sensible, chemical, or

medical properties. Nevertheless one particular species has been always preferred for culture, with the view of making opium; and that species is the *P. Somniferum*, called generally white poppy, from the more usual colour of its flowers, or from the whiteness of its seeds. The preference given to this species, for this purpose, arises solely from its more luxuriant growth, and larger capsule or seed-vessel; which, in consequence of its greater comparative bulk, may be made to yield a larger amount of opium than any of the other kinds, the capsules of which vary in size from a hen's egg to a garden pea.

The white poppy is a native of the fields, and waste grounds in the South of Europe, and of the same parallels of latitude throughout Asia. It was early introduced into England and has even been said to be found there indigenous: certain it is that it bears the temperature of that country well, and has been there profitably cultivated. In the year 1796, the "Society for the encouragement of Arts" granted premiums of 50 guineas each, to two gentlemen for their success in raising British opium.* Experiments made in the United States fully warrant the conclusion that the Poppy plant may be advantageously cultivated even in the northern States;† and in the southern become a source of considerable revenue. Some trial has lately been made with it at the Shaker settlement in this neighbourhood; and although not extensively pursued, the result was altogether encouraging; the opium produced being of the purest and finest kind; yielding on chemical analysis a greater amount, in a given quantity, of the active constituents of the drug, than the common Turkey samples.

The Poppy will grow in almost any soil, but it flourishes best in rich, loamy ground, well manured with thoroughly rotted dung, and frequently ploughed or turned so as to render it mellow. Towards the 1st of March, or during the month of February, if the ground be sufficiently dry, the seeds should be sown. For the purpose of collecting the opium most conveniently, this is done in shallow drills, eighteen inches or two feet

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* Annals of Medicine for 1796. † Medical Repository, vol. i, and vol. iii.

apart, strewing the seed very thinly in them: the young plants are not easily injured by frost, but do not bear transplanting well. About the middle of April, or when the plants are two inches high, they should be thinned, so as to leave them ten or twelve inches apart in the drills; and then frequently weeded, and occasionally watered, if the season be dry. In this manner they grow luxuriantly; each plant producing from four to ten flowers, and each flower being succeeded by a large capsule, or *head* as it is called. When these heads are fully grown, but whilst they are still green, the opium is obtained from them, by making slight incisions into them. This is best done with a small instrument, somewhat like a scarificator, having three or four cutting points, which will make as many incisions through the exterior epidermis, without penetrating the cavity of the head. With this instrument a person passes along the rows, and scarifies each ripe poppy head *on one side*. As soon as the incisions are made a juice of milky whiteness exudes, sometimes so abundantly as to drop on the leaves below, but more commonly it merely trickles to the bottom of the cut, where adhering it soon becomes brown and viscid. This operation should be done in the morning; and in the evening of the same day the small adherent masses of opium are scraped from the sides of the heads with the blade of a knife, or more conveniently, with a piece of iron having a circular edge, by which the exudations from all the cuts may be removed at one application. On the ensuing morning the opposite sides of the heads are treated in the same way, and in the evening, the produce being gathered, it is put with the previous parcels into a porcelain, or earthen dish, and exposed to the action of the sun until it attains the requisite spissitude, being occasionally worked up with a wooden spatula.

This is the process which from all testimony is pursued in Turkey: it is that which has been most successfully followed in the experiments which have been made in England; and that by which, no doubt, the purest opium is obtained. It is but proper to remark, however, that a somewhat different plan has been adopted, and recommended, by a gentleman of New York, which is here given in his own words.

"I have tried the Asiatic method, with several others; but none have ever succeeded so well with me as, in a sunshining day, to cut off the stalks at about an inch distance from their flowers, or capsules, and, as soon as the juice appears, which it does, at first, equally well on that part of the stalk cut off with the capsule, or flower, as on the standing part, to collect it with a small scoop or penknife, the last of which I have found to answer the purpose very well. After the juice ceases to appear on the top of the standing stalk, it should be cut off about an inch lower, when it will be found to yield almost as freely as before, and repeated as long as any juice appears.

"Since which, as it had been suggested that the method I recommended was not equal to the Asiatic, I made a number of experiments, to determine the matter with greater certainty: but what is necessary to add, and which is sufficient for the present purpose, is, that in every experiment, made under equal circumstances, I obtained the largest quantity in the former way, that is, by cutting the stalk, though it may require rather more care and labour."*

The harvest of opium-making lasts but for a short time, as the plants all flower and mature their heads sufficiently in a few weeks; the labour of gathering it, however, is so light that children, capable of exercising the proper amount of carefulness, are able to perform it. How far the product of one season may be augmented, by having different parcels of the plants to come on successively, remains to be determined by experiment.

Particular care should be taken not to permit any other species of the Poppy to grow in the neighbourhood, whilst cultivating the *P. Somniferum*, since it becomes easily adulterated, and even with the best care is apt to degenerate. Hence a few of the largest and first heads should be reserved for seed, and not tapped at all, and every few years the stock should be renewed from fresh parcels of seed.

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* Experiments on the cultivation of the Poppy-plant, and the method of procuring opium, &c. By Dr. Shadrach Ricketson, in the New York Medical Repository, vol. i, and vol. iii.

ART. VI. *Cases of Herniæ in which the Stricture remained at the mouth of the Sac, after they were returned, by the Taxis, into the Abdomen.* By Dr. N. SNEAD, of Washington county, Virginia.

THERE is frequently as much achieved for the interests of Medical Science, by reporting usuccessful cases as those that terminate successfully. They serve as guides, in conducting the practitioner to fortunate results in his subsequent practice. Of all the morbid conditions to which the human system is liable, strangulated hernia is amongst the most important. The rapidity of its march to a fatal termination, the intolerable suffering of the patient, and the embarrassment experienced by the surgeon, in his efforts to succeed without an operation, combine in an eminent degree to proclaim it one of the most interesting diseases that falls within the province of surgery. Consequently cases that present a striking aberration from those which are recognized by the greater number of physicians in practice, cannot fail to attract attention.

Case 1st. In the month of April, 1827, a negro man, the property of Mrs. Davis, whilst loading a waggon, forced a small hernial tumour into the right groin, which he with difficulty, reduced himself, by assuming a recumbent position. This patient had a reducible hernia on the left side, for which he wore a truss; though it occasionally protruded and gave him much pain and inconvenience, before he could succeed in its reduction. The case under consideration, on the right side, now presented itself for the first time. And, notwithstanding the hernial tumour had been returned into the cavity of the abdomen, symptoms of incarcerated bowel continued.

Dr. Hannum, of Abingdon, Virginia, was called in on the 4th day; the case still presenting the prominent characteristics of strangulated intestine, although not the slightest manifestation of a hernial tumourcould be perceived. It was stated to-

Mrs. Davis that the case afforded a strong presumption that the bowel was returned into the abdomen, in a state of strangulation; though as there was no tangible evidence of that fact, it was deemed most proper to rely on those remedies calculated to meet the prominent symptoms. Blood-letting, cathartics, blisters and enemata were resorted to, under the apprehension that it might be a case of constipation, associated with inflammation of the bowels. This course was prosecuted two or three days, without the slightest alleviation. I was requested to visit the patient on the 7th day of the disease, at which time he was labouring under all the constitutional symptoms of strangulated bowel, in addition to the supervention of peritoneal inflammation; frequent vomiting, a tense and exquisitely painful abdomen, obstinate constipation and hiccup, were the prominent features appertaining to the case. His pulse did not respond to the amount of disease presented, as it was very little altered from a natural state, either in point of force or frequency. We did not feel justifiable, from the obscurity of the case, to make an opening into the belly, at the internal abdominal ring and search for the strictured bowel; as it was evident that if an incarcerated intestine did exist, and was released by an operation, it would avail the patient nothing at so advanced a stage. Moreover, we were in possession of neither precedent nor authority that sustained us in performing an operation, that had for its object the exploring of a part of the abdomen, in search of a strangulated bowel, the existence of which was rendered doubtful, from the analogy of the symptoms in the case to those of other intestinal affections.

The negro died three days afterwards. A post mortem examination exhibited the following appearances. The peritoneum presented the highest grade of inflammation; adhesions had taken place throughout the whole range of the bowels, from the secretion of coagulable lymph. At that part of the abdomen corresponding to the internal abdominal ring, there was a small hernial sac, entirely within the cavity of the belly, containing about an inch and a half of small intestine; adhesions had contracted between the mouth of the sac and the bowel; the intestine embraced by this small sac was mortified.

Case 2d. In March, 1828, Mr. M'Collough, aged 18 years, in making an effort to raise a heavy burden, protruded a hernial tumour through the inguinal canal into the groin. Dr. Hannum was called to this case, and succeeded in reducing the hernia by the taxis, without the aid of relaxing agents. But the reduction of the hernia afforded no relief to the patient, as manifestations of incarcerated bowel still continued. The Doctor entertained an apprehension that his condition was perfectly analogous to that of Davis's negro: he, therefore, suggested the indispensable necessity of resorting to an operation, without delay. The patient and his relations hesitated; in as much as there were no apparent exhibitions of hernia. The former case was detailed in their presence, for the purpose of bringing about an acquiescence to the operation. The symptoms progressed; and I was requested to assist in performing the operation, on the third day of the disease, though, as I lived twenty miles from the residence of the patient, I could not arrive there until the morning of the fourth. The symptoms, then exhibited, were frequent vomiting, hiccup, tumid abdomen, unrelenting constipation, with pain and soreness in the region of the internal abdominal ring. The pulse was soft, full, and very little increased in frequency. It was stated to the friends of the patient that the success of the operation had become doubtful from delay, nevertheless, as it offered the only resource, it ought to be performed. I ascertained from the young man's mother that he had had a reducible hernia on the same side when quite young, though he had been entirely free from it for many years.

The operation as for hernia, wherein the seat of stricture is at the internal abdominal ring was first performed: then the cavity of the abdomen was opened by cutting through the internal abdominal ring and peritoneum, in a direction to avoid the epigastric artery. Thus the sac was exposed, and the bowel relieved of the stricture constituted by peritoneum.

The neck of the sac had not contracted adhesions to the bowel, as in the case of Davis's negro, though it was somewhat thickened by inflammation. The bowel was removed from the abdomen for examination; there was about two inches and a

half of small intestine that had been embraced by the hernial sac: it exhibited a very dark, red appearance, approaching to gangrene; its circumference, at that portion which had suffered the constriction, was greatly contracted. We met with some difficulty in deciding upon the propriety of returning the bowel into the abdomen, or leaving it in the wound; though as it was not absolutely gangrenous, it was returned. The sides of the wound were approximated by the interrupted suture and adhesive strips. In a short time the patient had copious and spontaneous evacuations from the bowels, attended by a relief of the vomiting and other harrassing symptoms. The day after the operation he and his friends anticipated a speedy recovery, as he was tranquil and free from pain. I need not detail the symptoms subsequent to the operation, as they were not remarkable. He died on the 4th day after the operation, no doubt, I presume, from mortified intestine, as that progressive sinking of the vital energies came on in a day or two, that is so peculiarly characteristic of gangrenous mischief, where it results to textures important to life.

These cases present a peculiarity in result, that has not come within the range of my professional inquiries. Professor Dudley states in his lectures, that in a large majority of the operations which he has performed in hernia, the stricture was formed by the mouth of the sac; and there is no difficulty in conceiving the practicability of returning a hernia into the abdomen, by the taxis, in this condition, exhibiting pathological results analogous to those which have been the subject of detail; although I have not seen any such communications. We see cases recorded in which symptoms of strangulated bowel were present, without any external appearances of a tumour; though upon dissection the obscurity of the cases was removed, when a small hernia was found strictured by the inferior edge of the transversalis muscle, at the internal abdominal ring, the bowel descending no further. In the cases of Davis's negro and McCullouch, the hernia descended into the groin, and was returned entirely within the abdomen, the bowel still remaining incarcerated by the mouth of the sac.

In performing an operation for any species of strangulated hernia, I would invariably open the sac and ascertain whether its neck formed the stricture or not. I should not think of adopting the plan, although recommended by distinguished authority, of returning the bowel, without opening or even exposing the sac. The hazard of returning the hernial sac into the abdomen, with the neck stricturing its contents, would be an insuperable objection to the prosecution of such a course of practice. The obstacle offered to the reduction of an inguinal hernia may be constituted by the tendon of the external oblique muscle; but that circumstance affords no evidence that the neck of the sac does not maintain its contents in a state of strangulation. Though, if it were practicable to return the contents of the sac, without returning it at the same time, the practice of not opening the sac would be safe, provided we were certain that its contents had not sustained an irretrievable injury.

ART. VII. *Case of retained Placentæ, one of which continued to grow thirty-eight days after miscarriage.* By G. F. H. CROCKETT, M. D., of Woodford county, Kentucky.

MRS. C—, æt 26; of sanguineo-bilious temperament; plethoric habit, and healthy; was married in May, 1824. July 4th, 1826, she aborted at the end of the 4th month. She was not pregnant before, or after this, till 1829: July 5th of this year, at 4 P. M. she miscarried at the end of the 10th week: 22 hours after a second child passed: not more than an ℥j of blood was lost in the whole affair. The after-birth not appearing, her friends felt some apprehension for her safety. I endeavoured to quiet them by telling them that it was sometimes retained for several days; that interference would be improper in her case, unless other symptoms than those present should make their appearance: and that I would attend her closely, and would, I

believed, be able to guard against danger. I advised Mr. C. to consult an experienced accoucheur in relation to her case, who concurred with me in opinion.

In two or three days after the accident, she was almost as well as before, and was soon able to go about as usual. I directed her to keep her bowels constantly a little laxative, by the use of pills, principally aloetic, and to use, twice a day, injections, per vaginam, of camomile infusion, ascidulated with the hydrochloric acid. I saw her frequently for two or three weeks, during which time she appeared to be entirely well, except, as she expressed herself, the uterus appeared very nearly, if not quite, as large as before the miscarriage. Supposing that in this she might be mistaken; that the secundines, being small, might have passed unperceived, I ceased my attendance, merely directing her to continue the course previously laid down. She attended to her business, exercised, rode on horseback and in gig, as usual.

In the afternoon of August 9th, a slight red discharge appeared, which she supposed to be the return of her catamenia; it gradually increased, and on the 11th was accompanied with some pain. At 4 o'clock P. M. on the 12th, I was summoned in haste, and found her with violent pains and considerable flooding. She had been put in a proper position; refrigerants applied, and the solution of sulph. alumine administered without effect. She was yet strong in muscle and pulse. I took ℥xiv. of blood from the arm, which abated the force of the latter, and gave her acet. plumb. grs. xv. The flooding was very much lessened in ten minutes, and in twenty-five or thirty after, the lead causing sickness, she vomited, and while in the act, "the long looked for" placenta passed, entirely healthy, natural, destitute of fœtor, and as large as it would have been had the process of fœtal gestation continued without interruption until that time. No vestige remained of the point of insertion of the funis umbilicalis; the arteries and veins seemed to have inosculated, and a perfect circulation to have been established and kept up throughout the placenta. The membranes seemed to have sloughed off. The flooding soon after ceased.

entirely, she rested well during the night, and was quite smart next morning.

Twenty-four hours after the appearance of the first, she was, without the least pain or interference, delivered of a second placenta, which also was healthy in appearance and destitute of fœtor, but very small. From this time she did well and had a speedy recovery of her usual good health. The placentæ being washed clean and immersed in diluted alcohol, were weighed twenty-four hours after their delivery; the first weighed 21 drachms, and the last 100 grains.

From the circumstances of the case, it would seem probable that there was no rupture of the vessels between the placentæ and the uterus at the time of miscarriage, and that the small quantity of blood lost was discharged at the rupture of the cords. From the period of the miscarriage to that of the delivery of the placentæ, there was sometimes hardness of the breasts, alternated with flaccidity and considerable secretion of milk.

During both pregnancies, Mrs. C. was much troubled with constipation; and from the commencement to the interruption of the last, with unusually severe gastric irritation, which produced much sickness and vomiting.

Art. VIII. *Case of Consumption, relieved by truncating the Uvula.* By Augustus W. Mills, M. D. of Winchester, Kentucky. *Communicated in a letter to Professor Caldwell.*

Dear Sir:—In the course of your lectures delivered in the winters of 1826–7—1827–8 you suggested the propriety of cutting off the Uvula with a view to cure a certain description of Consumption, which is the result of an elongation of that muscle. I have recently been led to perform the operation; and it is to you that I feel indebted for the credit which the successful termination of the case has given me.

The subject is a negro man aged 32 years, the property of Dr. Field, a medical gentleman of this neighbourhood. He had been affected with a most distressing cough for the last six or eight months; so much so that sleep could only be procured by large and frequently repeated draughts of laudanum. He had undergone the usual treatment for phthisis pulmonalis, had been purged, bled copiously and blistered frequently; but he did not derive the least benefit from any of the above mentioned remedies. He applied to me about the middle of April, at which time he was very much emaciated; and it was with considerable difficulty that he could walk unsupported the distance of fifty yards. After giving me the history of his case, I was at a loss to know what course to pursue. I gave, however, some alterative medicines, and directed him to return on the day following. He had not left my room long before it occurred to me that his disease proceeded from an elongation of the uvula. He returned on the next day, and, upon examination, I found the uvula hanging down on the root of the tongue. Judge my gratification when I discovered the cause of his disease, and knew that by a very simple operation I should cure him, contrary to the expectations of every one who had seen him. I determined to cut off that portion of the uvula which produced the cough, and immediately proceeded to its excision, which I affected with little difficulty, having no other instruments than the forceps and curved scissors. I took off about three fourths of an inch of the diseased uvula, and about half an inch more sloughed off afterwards. In the course of ten days my patient was entirely restored to health. He has had no cough from the time of the operation, and a course of the blue pill entirely removed the constitutional disturbance of his system.

That portion of the uvula which I took off, had very nearly the firmness and consistency of cartilage. The blood lost by its removal, would not amount to a table spoonfull.

June 4th, 1829.

ART. IX. *Notice of an Epidemic Dysentery, which occured in certain parts of Alabama in the summer of* 1829. By TURNER D. BELL, M. D. of Springfield, Greene county, Alabama.

DURING the month of June last, there prevailed through this country, an Epidemic Dysentery, which was fatal to many, in its course, and of which I design to give a short account.

The face of the country where this alarming disease was most prevalent, is flat, low, and marshy; and is permeated by a considerable number of water courses, whose supplies are usually exhausted by the ordinary droughts of the summer months, but upon which are, nevertheless, erected mills, cotton-gins and other machinery. The population is dense, and cotton is cultivated very extensively.

This is the general description of all that country between the *Tombigbee* and *Black Warrior* rivers for 35 or 40 miles above their junction, and a particular one of the country eastward of Springfield on the opposite side of the Black Warrior river. The former is denominated the "Fork," the latter the "Flat Woods."

In these two localities the disease prevailed more violently than any where else in this section of the country. The last spring having been an unusually wet one, and the country having been much inundated by rains, succeeded by hot weather, a considerable portion of vegetable matter was necessarily flooded together, and of course happily situated for rapid putrifaction, and the evolution of miasmata. The first effects of these causes upon the inhabitants of this vicinity were shown in the forms of cholera morbus and cholera infantum. These were very severe but soon disappeared. The dysentery then made its appearance, together with a congested fever; the former, however, was the most prevalent of the two.

This disease was ushered in by various symptoms. Some-

times it came on with a chill and rigor of the whole system: at other times with a sense of griping, and a discharge of blood and mucus: by a febrile state of the system, attended with a burning sensation of the abdomen, headach and great thirst; the pulse being generally small, hard and frequent. After the disease had continued a day or two, the patient complained of great thirst and burning in the hands and feet: others would have a coldness of the extremities, attended with the secretion of a clammy moisture; vomiting was unfrequent; it occured nevertheless in some cases.

The treatment, as might be expected in a wide spreading and alarming epidemic, was various and often empirical. I will barely mention the different plans adopted by other physicians and pursued in domestic practice, together with that which I found most beneficial.

In the outset of the disease, castor-oil and laudanum were resorted to, together with mucilages of different kinds; and the lancet was generally used as a preparatory measure to this course of treatment. Antimonials of various kinds were also used. Small doses of calomel repeatedly given were tried, together with laudanum and tincture of kino. The purging neutral salts were used in some cases; and blisters were very commonly applied, together with sinapisms. Of these, castor-oil, laudanum and mucilages constituted the most common practice, which was generally unsuccessful: in more than half the cases—in truth I believe in all the severe and stubborn cases, treated by castor-oil and laudanum, the issue was fatal. In the more mild form where the biliary ducts were not locked up, this practice succeeded very well. But I will here remark, as the substance of my experience with this so commonly employed article in dysentery, that in bad cases of that disease, it will not only be found inadequate to the cure, but positively injurious. I have frequently observed that soon after drinking the oil, a burning sensation in the stomach and intestines was experienced; this burning sensation continued and the pains were even increased after the operation of the oil was over.

Calomel in large doses, combined with opium or laudanum,

(according to the age of the patient,) assisted with the warm bath, and followed by the tincture of catechu or kino, mixed with mucilaginous drinks, constituted another plan of treatment, and was that which, with some modification, I generally adopted. The practice of giving large doses of calomel, and repeating them frequently, was at first much cried down and denounced, until its success was so universal, that its efficacy was generally acknowledged. Out of about 40 cases of this dreadful malady, which came under my care, only two proved fatal; one of which was from imprudence in taking a large draught of cold water, and the other from a defect in constitution.

At one place, to which I was called, there were twelve patients, all prostrate and helpless, not one being able to minister aid to another. They had a constant inclination to go to stool, and a discharge of bloody froth came at every effort, attended with high fever, great thirst and burning of the hands and feet; their tongues were foul; they manifested great anxiety of mind; and despair hung heavily on all of them, for their attending physician not being able to check the disease, had finally abandoned them to the same fatal destiny which had overtaken three of the family a few days before. They were all immersed in a warm bath, which produced composure and perspiration: a dose of calomel, varying from 20 to 40 grains according to their ages, was then exhibited to each, which produced dark, bilious and offensive discharges, with great relief of all the symptoms. In twelve hours, there being some return of tenesmus and bloody stools, the bath was again used and calomel in similar portions repeated, followed by laudanum and catechu; and in a few days they were all convalescent.

In *all cases* where calomel was given in small doses the disease was augmented and proved fatal; and this practice of course was soon abandoned.

When castor-oil was given in the malignant forms of the disease, it increased it, proving highly injurious and frequently fatal.

The number carried off by this disease has never been equaled here by the regular bilious fever, in the same length of time.

ART. X.—*An Essay on the History, Causes and Treatment of Typhus Fever.* To which the Annual Prize for the year 1828, was awarded by the Medical Society of the State of New York. By ALFRED T. MAGILL, M. D., of Winchester, Virginia.

(NOTE. The following Essay was printed in the New York Medical and Physical Journal for July 1829. But as that able periodical has only a limited circulation in the South and West, where this is chiefly taken, we republish it, (though somewhat at variance with our custom,) for the purpose of giving as extensive a reading as possible to so valuable a paper. We regret not having received it in time to give it a more prominent place in this number, and that our limits will compel us to divide it.—EDS.)

"Ex principiis nascitur probabilitas; ex factis vero veritas."

THE word Typhus, according to most writers, is derived from the Greek noun *tuphos*, stupor, a symptom which frequently supervenes, sooner or later, in the progress of the disease; oftener, however, coming on towards its termination, than existing in its earlier stages. The learned and scientific Good, however, refers the derivation of the term to the verb *tupho*, which signifies "to smoulder," or "to burn and smoke without a vent," and thinks it admirably "expressive of the general nature of the fever to which it was applied at first, and which it designates at present; which burns not with open violence as the cauma, but

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Medical Society of the State of New York.—The committee on prize questions and dissertations, consisting of Drs. Manley and Watts of New York, and Drs. Eights, T. Romeyn Beck, and James M'Naughton, of Albany, have unanimously awarded the premium of $100 for the best essay on *typhus fever*, to the dissertation having the motto "*Ex principiis nascitur probabilitas, ex factis vero veritas.*"—On opening the sealed note accompanying, the successful candidate was found to be Alfred T. Magill, M. D., of Winchester, Virginia. By order.

JOEL A. WING, Secretary.

with a sort of concealed and smothered flame."* There are, however, various other appellations by which this fever is known; such as the jail, camp, hospital, and ship fever. It has also been denominated the putrid, malignant fever, and from the frequent presence of petechiæ it is called, on the continent of Europe, febris petechialis, and constitutes the contagious fever of Lind, the febris inirritativa of Darwin, and the nervous fever of other writers.

The history of this fever is rather obscure. The ancient authors on medicine make no particular mention of it, from which we may conclude, that they were either unacquainted with it, or, considering it merely as a variety of the common fever of their respective countries, did not deem it worthy of special notice. Sydenham was probably the first medical writer who described this disease, not, however, by the name under which it is now known. The account of it is contained in his "Essay on the rise of a new Fever." He says, "As soon as it began to thaw in February, 1685, the fever I intend to treat of here appeared, which I esteemed a new sort of fever, and altogether unlike the fever of the eight foregoing years."† No one can read the symptoms of that fever as narrated by that great and good man, without remarking the striking similarity between them and the assemblage of symptoms which constitute the typhus of the present day; and, indeed, it has been considered as such by subsequent writers.‡ Sydenham does not mention that this fever was at all contagious, but attributed it to a particular state or constitution of the atmosphere.

In the year 1577, more than one hundred years anterior to the date of this fever described by Sydenham, a disease suddenly made its appearance, during the Black Assize at Oxford, in the month of July. This fever is generally cited as an instance of jail fever, and as affording incontestible evidence of

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* Good's Study of Medicine, vol. 2, p. 123.

† Wallis's Syd., vol. 2, p. 315.

‡ Wallis, in one of his notes to this essay, says, "Dr. Cullen considers this as a species of typhus, but of the milder kind, and arranges it under that genus by the name febris nova."

its contagious character. The infection in this instance was supposed to have been communicated by one Rowland Jencks, a criminal, who "was arraigned and condemned in the presence of *a great number of people to lose his ears. Judgment being passed, and the prisoner taken away*, there arose such an *infectious damp* or *breath* among the people, that *many there present, to the apprehension of most men, were there smothered;* and others so deeply infected that they lived not many hours after."* This fever is no doubt one of the instances alluded to by lord chancellor Bacon, when he says, that "The most pernicious infection, next to the plague, is the smell of the jail, where prisoners have been long, close, and nastily kept; whereof we have had in our time, experience twice or thrice, when both the judges that sat upon the jail, and numbers of those who attended the business, or were present, sickened upon it and died."

Nearly nine years subsequent to this event, an infectious fever (as it was called) made its appearance at Exeter, during the assizes there, in March, 1586. This fever was attributed by many to contagion communicated by some Portuguese seamen, "who had been sometime before taken at sea, coming with fish from Newfoundland," and "cast into the deep pit and stinking dungeon" of the "goal of the castle of Exon;" and had no change of apparell," but being left to lie "upon the ground without succour or reliefe, were soon infected."† These persons, when brought into court for their trial, were supposed to have communicated the contagion to many of the individuals present.

"From that time (says Bancroft) I can discover no instance of any remarkable mortality or sickness, supposed to have been produced by jail infection, until the year 1730, (an interval of one hundred and fifty-three years,) when, at the Lent assizes, some prisoners, who had been removed from Ilchester jail, to take their trial at Taunton, were believed to have infected a part of the court, and produced a contagious disease"‡ of which many died.

Huxham, also, mentions in vol. 2, p. 82, of his work de

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*Bancroft on Fevers, p. 430. †Bancroft, p. 434. ‡Ibid, p. 110.

Aere, &c., that a disease, which he calls "Febris putrida, contagiosa ac pestifera valde," broke out at Launceton, in the month of April, 1742. He says, "genita hoc in carceribus febris et per comitia provincialia disseminata longe lateque."

In the year 1750, a fever made its appearance during the sessions of the Old Bailey, which proved fatal to the lord mayor, and two of the judges, with several eminent and other persons, who, as was asserted, and is now generally believed, were infected by the contagion of jail fever brought into the court from Newgate."*

Previous, however, to this occurrence at the Old Bailey, two essays had appeared on the subject, from the pen of Ebenezer Gilchrist, of Dumfries, which were published in the fourth and fifth volumes of the Edinburgh Medical Essays and Observations. He called it "nervous fever." Neither its origin nor its propagation was attributed by him to contagion.

A considerable time after this, Huxham and sir John Pringle published their essays on typhus—Huxham under the appellations of "slow nervous fever" (febris lenta nervosa) and "malignant putrid fever,"† and Pringle under that of "jail or hospital fever." Both these writers were strenuous advocates for the attribute of contagion as belonging to this fever. Subsequent to these publications we find much more attention paid to this subject, and its pathology and causes have been investigated with much ardor and diligence. Many of those who have written on it have contended for its being contagious; some there are, however, who dissent from this doctrine, and deny that it possesses any such property.

This fever has been divided by nosologists into the *typhus mitior*, (mild typhus or nervous fever,) and *typhus gravior*, (malignant or putrid typhus;) thus indicating its sometimes mild and sometimes violent character. This distinction appears to us to be entirely useless. It produces an unnecessary multiplication

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* Bancroft, p. 111.

† The "slow nervous fever" and the "malignant putrid fever', of Huxham, are obviously only different grades of the same disease; they correspond to the typhus mitior and gravior of nosologists.

of words; the same language descriptive of one grade being applicable with a slight variation, to the other; so much so, that both can be easily comprehended in one general description; and it, moreover, leads to no good practical result. The typhus mitior and gravior being only grades of the same disease, are to be treated exactly on the same principles; and that physician who has not sagacity enough to discriminate between a mild and a severe fever, will not have his intellectual vision brightened by such distinctions. Independent, however, of these reasons, we do not think, in practice at least, that the distinction can always be made. However easily it may be done in the closet, it will be found often impossible at the bedside of the patient. Every physician of much experience in this disease, will acknowledge that, in a majority of cases, the symptoms are such as to render it doubtful to which grade they belong. Wilson remarks, that "between the worst and mildest forms, there are innumerable degrees which insensibly run into each other;" thus rendering it impossible to draw a distinct line of demarcation between them. Those, therefore, who are in favor of distinguishing typhus into grades, should carry out this principle, and give a separate description of each of its "innumerable degrees;" for there would be just as much ground for this, in a practical point of view, as there is for the other; they should also extend this principle to other fevers, for if this distinction be necessary in typhus, it is equally so, for instance, in yellow fever, for it also has its gradations of violence and mildness. Another objection to it is this: the different grades of this fever are so readily convertible into each other, that, according to the treatment pursued, what was mitior to-day, may be gravior to-morrow, and *vice versa*. For these reasons we reject these distinctions, which we consider arbitrary and useless.

Armstrong, in his unequalled work on typhus fever,* notices

* We cannot mention this work, without expressing the unbounded admiration we feel for the talents of him, who composed it, and our conviction of its great and inestimable value. If we were required to put our finger upon a volume, containing the greatest amount of practical information on the diseases of which it treats, we should unhesitatingly select it in preference to any work with which we are acquainted. The neatness, elegance and perspi-

three varieties instead of two. He divides the disease into the typhus of simple excitement, the inflammatory and the congestive typhus; though we have the same objection to his two first divisions, that we have to those of mitior and gravior, yet, nevertheless, we think his arrangement on the whole better than the other; the phraseology which he employs to designate the different forms of this disease is, we think, preferable to that of mitior and gravior, inasmuch as it expresses in itself the real character and pathology of the disease, and strikes at the root of those notions of debility and putrescency as prominent features of this fever, which we derive from books, and which are intimately associated with all our reflections on it; and it moreover includes a form of the disease, which unquestionably demands a separate consideration, i. e. congestive fever. Until the publication of his work, we had no distinct conception of the pathology of congestive fever; his post mortem examinations, however, have revealed its true character; and his graphic pen has delineated with so much accuracy its symptoms, that it cannot fail to be recognized by even the most careless observer. But though we prefer his arrangement to the other, we shall adopt it only in part. We will include his two first divisions in one general description, and then treat of congestive typhus, under a separate head. We will now proceed to an enumeration of the symptoms of typhus.

Symptoms. This fever commences with the same symptoms which usher in the attack of other febrile affections.* A feeling of langour and an indescribable sense of weariness and dejection, are usually the first links in the chain of symptoms which constitute this disease; there is also, usually, some slight gastric distress, sometimes amounting to nausea, together with a total loss of appetite. There is, in some cases, a cold creeping sensation over the body; in others, particularly when the

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cuity of its style, the vast fund of practical instruction spread over its pages, and the vital principle of genius which pervades every portion of it, has always rendered its perusal, to us, an intellectual treat.

* Pringle remarks, that "the disease in the beginning is not easily distinguished from any common fever."

attack is severe, this coldness amounts to rigor, alternating with quick and transient flushes of heat; the face is pale, with some slight appearance of livor under the eyes; and there is more or less of a load, pain, or giddiness of the head—the pulse at this time is generally weak, low, and frequently irregular. This assemblage of symptoms constitutes what has been aptly termed by Armstrong the stage of oppression. This may continue from a few hours to as many days; sooner or later, however, evidences of decided reaction supervene. The face loses its paleness and generally becomes much flushed; the pulse increases in frequency; in some cases it is tense and hard; the head-ache becomes excruciating, and the mind is often overwhelmed with delirium. The skin grows very warm, and the heat appears to have a penetrating quality.* The thirst becomes considerable, and, in some cases, almost unquenchable. In milder cases these symptoms are much less prominent and severe: in such cases, the pulse, though quicker than natural, is neither hard nor strong, and the uneasiness of the head not near as great; the delirium, if present, is seldom violent and uncontrolable as in the severer modifications of the disease. Petechiæ also frequently make their appearance, though they are by no means peculiar to typhus, but frequently occur in other fevers; the mouth is parched and dry, and the tongue, from the beginning, is more or less furred, and, in the progress of the disease, generally brown, and sometimes black; the bowels are often much disposed to constipation, and the secretions and excretions are much changed; the alvine discharges becoming dark and offensive, and the odor of the breath and body very unpleasant. After a longer or shorter time, varying from the

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* Pringle notices this symptom. "On first touching the skin," says he, "the heat seems inconsiderable; but upon feeling the pulse for some time, I have been sensible of an uncommon ardor, leaving an unpleasant sensation on my fingers for a few minutes after. The first time I observed this, I referred it to the force of imagination, but was assured of the reality by repeated experiments, and by the testimony of others, who, without knowing of my observation, had made the same remark." Other physicians have also mentioned it. Frank, in allusion to it, says, "sæpe manifeste acer digitosque urens." It has been called by others a biting heat. It is not, however, peculiar to typhus, as we have observed it in other fevers.

seventh to the eighteenth day, and, in severe cases, sometimes a much shorter period, a change takes place in the character of the disease; what Armstrong calls the stage of collapse supervenes, and if the patient has been properly treated during the period of excitement, it is usually the beginning of a return to health, which is indicated by an abatement of all the symptoms; but if (particularly in severe cases) the fever and topical inflammation, which are almost always present in such cases, have not been kept in proper subjection, it is a change pregnant with the utmost danger to the patient; the delirium is lost in profound stupor, the tongue becomes exceedingly black and dry; dark sordes collect about the teeth; subsultus tendinum, and what have been denominated symptoms of putrescency, appear; the petechiæ, if present, turn to a dark purple color; hemorrhages from the stomach, bowels, and kidneys, (the last making the urine dark,*) come on, or "profuse ichorous and involuntary stools" soon extinguish the remains of life. The patient, however, is sometimes carried off by convulsions.

Prognosis. In forming our prognosis of the probable event of this fever, we must not deduce our opinion from any one symptom, however prominent it may be, but rather from mature deliberation upon the symptoms taken collectively: as the chief danger, not only in this, but in all fevers, arises from the disorganizing effects of high and uncontrolled excitement, upon some one of the vital organs, it follows, that if this be moderate, or if it be even high, and we find that we can keep it in due subjection, then we may rationally anticipate a favourable termination; but if neither of these conditions obtain, then we may justly dread the event. But to be more particular: we may calculate on recovery when the delirium is slight, the strength not much impaired, the tongue moist and beginning to clear, bowels loose and easily operated on by medicine; when deglutition is performed without difficulty, and the respiration is free

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*Huxham says, "I have frequently seen the urine, in petechial fever almost black."

and easy,* with a soft and moist skin. A deafness coming on during the fever is considered by many a favourable sign.†

Besides the signs already mentioned, there are what are called critical discharges. Among the most frequent of these, is sweating; this evacuation was at one time considered of so much importance in the cure of fever, that it was thought a *sine qua non* to recovery,‡ and accordingly the most heating and pernicious means were employed to force this discharge. This destructive and irrational practice is fortunately almost exploded at the present time, and physicians now, with the exception of using a few antimonials, generally leave it to nature, who effects it best in her own way.§

The most frequent critical discharge is a diarrhœa; this, when of a proper kind, very often terminates the fever; it is always most beneficial when the stools are bilious and somewhat consistent; profuse liquid and colliquative discharges always do harm, and often irretrievably prostrate the patient in a very short

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*Fordyce on Fever, page 221, observes, that, "although the patient should be insensible to all external objects, although he should sleep very little, yet, if the deglutition and respiration should remain unimpaired, the patient is not to be despaired of; it happens even that he most commonly recovers. But if he respires with great difficulty, or hardly at all, or if the deglutition be almost totally prevented, or if the attempt to swallow throws him into convulsive contractions, he rarely recovers."

†We have mentioned deafness as one of the favourable symptoms in typhus, more upon the authority of others, than on our own conviction of its being such; to us it clearly indicates a degree of fulness of the cerebral vessels, which, in our opinion, is much to be deprecated. That it is owing to this cause is plain from its being removed by evacuants, and by its returning if these are discontinued. Every physician must have witnessed this. Hippocrates testifies to this. In section 4th, aphorism 28th, he remarks, "Quibus biliosæ sunt egestiones, surditate superveniente cessant; et, quibus surditas est, biliosis supervenientibus cessat." Although in the first part of this aphorism, he seems to consider the cessation of the bilious discharges as the effect of the deafness, yet it is obvious from the latter part of it, in which he says this deafness is removed by the recurrence of these evacuations, that he had mistaken the cause for the effect.

‡Huxham declares that "he never once saw a malignant typhus cured, till more or less sweat had issued."

§To Sydenham the praise is justly due for having put down the pernicious practice of endeavouring to force a perspiration, by the use of the most heating and inflammatory measures, and of substituting in its place the cooling and evacuating plan of treatment. It constituted by no means the least of his triumphs over the deep rooted prejudices of the times in which he lived.

time. This last description of diarrhœa is, of course, to be immediately arrested; the first kind ought rather to be encouraged. Hemorrhages, likewise, often prove critical in this fever; the blood may proceed from the nose, stomach, or bowels. Persons have been often rescued from the grave, when given up as lost, by the opportune occurrence of hemorrhage from the bowels, and other parts; they should always admonish the practitioner to relieve nature from the necessity of resorting to this hazardous alternative, by using proper depletion in the first stages of the disease; we say hazardous alternative, because it often comes too late, and as often is excessive in quantity. Hemorrhage only does good in typhus when it occurs early in the disease, before the constitution has been shattered by the long continuance of the fever. This fact did not escape the sagacious mind of the father of medicine. In one of his aphorisms, he says, that "livid, bloody, fœtid, and bilious stools, coming on in continued fevers, are alike unfavourable; but a timely appearance of them beneficial."*

This fever also terminates occasionally in glandular swellings. Dr. Donald Monro, in his work on diseases of the army, observes, that "swellings of the parotid glands appeared in many subjects towards the decline of the fever, which came to suppuration and proved critical." Pringle, and other writers, also mention their occurrence, and recommend an early incision to discharge the pus.

Having enumerated the favorable symptoms, we will now briefly mention those which portend the approach of death. The bad signs are high delirium or deep stupor, subsultus tendinum, black, dry, and tremulous tongue, and inability to protrude it beyond the lips; difficult deglutition, and impeded respiration; weak, irregular, and intermitting pulse; great restlessness, evinced by frequent attempts to get out of bed, or by continual tossing from one side of it to another; the surface of the body bedewed with a cold sweat; frequent colliquative stools and hemorrhage from the mouth, ears, &c.; sometimes the pa-

* "Excretiones in febribus non intermittentibus lividæ, et cruentæ, et gravcolentes, et biliosæ, omnes malæ sunt. At probe secedentes bonæ.

tient expires in convulsions. Such is the dark catalogue of symptoms which declare death to be at hand. Even these, however, should not deter us from exertions to save life; persons sometimes recover with nearly all the above mentioned desperate symptoms, and the knowledge of this fact should teach us the important lesson never to abandon, as entirely hopeless, any case of fever; appearances are sometimes fallacious,* and we should never fold our arms in inactivity, though a combination of the worst symptoms should exist.

Pathology of Typhus. Typhus fever has been, for a long time, and still is with too many, associated with the ideas of debility and putrescency as its standing and prominent symptoms. The prejudices on this head, however, are beginning to lose some portion of their sway, in consequence of the flood of light which has been shed on the pathology and treatment of typhus, by some recent publications. Among these the work by Armstrong stands conspicuous; the appearance of which may be said to have constituted a new era in the history of typhus. But while bestowing due praise on this distinguished writer, let us not forget the illustrious Sydenham, whose sagacious and strong mind, more than a century and a half ago, discerned the true character of this fever, and suggested a course of practice, which, though in a great measure lost or forgotten amid the conflicting opinions of subsequent writers, has been revived in the present century, and is now, with but little variation, followed by all those who do not still hold to the doctrine of debility as belonging to this disease in all its stages. But to return to the subject. We will endeavour to show, from the various circumstances and phenomena incident to it, that, so far from being a disease of debility, it is essentially one of excitement, and frequently of inflammation as its consequence. One form of typhus, (i. e. the congestive,) will not, of course, be comprehended in this.

A state of real debility is a condition of the system which is generally, (we might say always,) the consequence of long con-

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* Hippocrates remarks, that "acutorum morborum incertæ admodum, ac fallaces sunt prædictiones."

tinued excitement, or the more gradual, though not less certain, result of an insidious chronic affection, preying upon some important and vital viscus. This *real debility* is always increased by such remedies as are termed evacuants, and, on the contrary, is diminished by such means as add tone to the system; but if, in the commencement of a fever, a person appears very weak, and if we find that the administration of tonics and stimulants only increases the debility, and, on the other hand, that evacuants, such as bleeding and purging, lessen it, the legitimate conclusion from these facts is, that this weakness is not real, but only apparent. This, then, is the case with the debility in the primary stages of typhus; it is increased by the exhibiton of stimulants and lessened by evacuating measures. Armstrong, whose experience was very great in this fever, bears testimony to this fact: "In the first stages of typhus," says he, "the debility is only apparent, and chiefly dependent upon the preternatural accumulations of blood in the veins about the head, heart, liver, and other internal parts, while there is less circulating upon the external surface of the body, than in a natural state. In the second stage, the debility is still only apparent, being then only the consequence of over excitement of the heart and arteries; but in the third and last stage, beyond all dispute, the debility is real, as it is then connected with a general collapse, which sooner or later succeeds to a state of febrile excitement, as certainly as exhaustion follows a fit of intoxication." And a little farther on he says, that though "this proposition is not evident at first sight, yet it is as certainly true as any in physic, and is proved from the debility in the first and second stages being increased by wine and cordials and lessened by a spare regimen and evacuations."*

Every physician must have witnessed the positively strengthening effects of cathartics in typhus fevers, before the constitution has been really debilitated by long continued excitement. A patient who has been carried to the close stool, after a copious evacuation from his bowels is able to get up, and walk without assistance to his bed.† This invigorating effect of purga-

* Armstrong on Typhus, p. 25.

† This is also frequently observed in other fevers. Dr. Bryce, in his account of the yellow fever on board the Busbridge Indiaman, in 1792, observes, that

tives in typhus and other fevers, is of frequent occurrence, and is totally unaccountable upon the supposition of its being a disease of debility; we commit, therefore an egregious error when we mistake this apparent for real debility. In the strong language of Rush, "the clown who supposes the crooked appearance of a stick, when thrust into a pail of water, to be real, does not err more against the laws of light, than that physician errs against a law of the animal economy, who mistakes the debility which arises from oppression for an exhausted state of the system, and attempts to remove it by stimulating means." But again, if this disease be one of great debility, there are many of its phenomena, mentioned by those who advocate this very theory, totally inexplicable, and indeed irreconcilable with this view of the disease. We presume every one will admit, that, if real debility be a characteristic of this fever, what are obviously debilitating discharges must increase this symptom, and, of course prove prejudicial to the patient; but that this is not the fact, we will endeavour to prove from the testimony of others. Wilson observes, that "a spontaneous flow of blood from different parts of the body, we have seen, is frequently attended with an abatement, or total removal, of the symptoms of synocha, and, what we should not *a priori* have expected, sometimes those of typhus;"* and in another place, when speaking of typhus, he says, "I have often seen them (i. e. hemorrhages) attend a general abatement of the symptoms."† We would ask Dr. Wilson how he can reconcile these facts with his notions of typhus fever?

The good effects of long continued and profuse sweating in this fever cannot be accounted for on the supposition of typhus being a disease of debility. We can well imagine that the loss of quarts of fluid might be salutary in a disease of high excitement, but it could never enter into our conception that the same loss would be beneficial to one who was really and *bona fide*

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he has "often seen a man carried upon deck perfectly delirious, with subsultus tendinum, and in a state of the greatest apparent debility, who, after one or two copious dark coloured evacuations, has returned to himself, and been astonished at his newly acquired strength."

* Wilson on Fever, vol. 1, p. 142. † Ibid. p. 134.

weak; and yet writers mention the good effects of several days' continuance of profuse perspiration.* A diarrhœa also, as was mentioned before, is often critical in this fever. We have thus endeavoured to prove from the very character of the debility in the first stages of typhus, and from the phenomena and circumstances incident to it, that the opinion of those who consider it a disease of genuine debility, and, as such, requiring tonic and stimulating remedies for its cure, is erroneous.

We will now take the opposite side of the question, and will endeavour to prove that it is a disease of excitement, and often of inflammation as its consequence. In the first place we will observe, that all the circumstances and phenomena of typhus already mentioned, and which are inexplicable upon the ground of its being a disease of debility, are easily accounted for upon the supposition of its being a disease of excitement and often of inflammation. The opposite effects of stimulants and evacuants in the first stages of typhus, the relief afforded by hemorrhages, profuse and long continued sweating, diarrhœa, and the abundant and free discharge of matter from grandular swellings, are perfectly consonant to, and exactly what we would expect from, this view of the disease. But we have still better ground for this opinion. Post mortem examinations prove it to be correct; they establish beyond doubt, that death in this fever is generally produced by the destructive ravages of inflammation on some one organ or organs of our frame. Many of the writers on this fever have recorded the results of their dissections. Pringle relates the appearances, on dissection, of ten persons who died of jail fever, in all of which, either inflammation or its effects were plainly visible; a majority of them had suppuration of the brain. Thomas says, "The usual appearances on dissection are, a softness and flaccidity in the solids; a dissolved state of the fluids, particularly of the blood; collections of sanious matter in the different cavities; turgescence and inflammation of the thoracic and abdominal viscera; and in the interior

* Monro, in his treatise on diseases of the army, remarks, that the sweating often continued, with the best effects, for several days. Hoffman, also, makes a similar statement.

parts of the brain increased vascularity and collections of serous fluid."* Armstrong mentions that his post mortem examitions always exhibited the appearances or consequences of inflammation. Dr. Dickson, in his account of the contagious fever which prevailed in the Russian fleet, observes, "that the appearances on dissection were strongly illustrative of the frequency of visceral inflammation, in the fever which he saw and described; for the brain, lungs, and contents of the abdomen were severally found to have been attacked among those who were the victims of this disease."† Ferriar also mentions the appearance on dissection of several persons who had died of typhus, all exhibiting the most unequivocal evidences of inflammation. These dissections seem to have made such an impression on him that he observes, after having mentioned the results of them, that "perhaps local inflammation is more frequently joined with typhus than we are aware."‡

Other accounts of post mortem examinations, corroborative of the above, might be mentioned, but these are deemed sufficient to establish, beyond doubt, the pathology of typhus, and that death in this fever is generally, if not always, the result of inflammation, disorganizing some important part of the animal machine. So universally, indeed, have the traces of inflammation been observed in those who die of this and other fevers, that many physicians have adduced it as the cause of fever in every instance. Clutterbuck, Ploucquet, Mills, Marcus, Reid,‖ and Broussais, are the great advocates for the local origin of fevers. The latter especially, a physician of eminent talents and much experience, has gained great celebrity by the ingenuity and ability with which he sustains his doctrine, and by the imposing array of talent which has flocked to his standard.

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* Thomas's Practice, p. 49. † Armstrong on Typhus, p. 154.

‡ Medical Histories, p. 76.

‖ Clutterbuck and Mills locate the inflammation, which they consider the cause of fever, in the brain. Ploucquet contended that fever was sympathetic of an inflammation of the brain, stomach and intestines; and Reid considered its seat to be the spine. Professor Marcus, of Bavaria, taking a more enlarged view of the subject, regarded all inflamed organs as equal causes of fever.

His doctrine is, that fever arises from an inflammation of the mucous lining of the stomach and bowels.

It would be irrelevant to the object of this essay to attempt any thing like a laboured refutation of this theory; which, enforced with all the "zeal, energy, and talents which this celebrated school can bring into the field," has spread itself extensively over the continent of Europe, and having crossed the waters of the Atlantic, can number among its supporters many of the distinguished medical characters of this country. We are not among those, however, who assent to the truth of this doctrine; on the contrary, we reject it unqualifiedly, as being, in our opinion, utterly at war with the true pathology and phenomena of fever. We do not believe fever arises from inflammation of the mucous membrane of the stomach for the following reasons. 1st. Because in many cases we have no evidence of its existence at the commencement of fever, or for even several days after; and not unfrequently during the whole course of the disease, we have no one symptom indicative of its presence. 2d. If inflammation of the mucous membrane of the stomach and bowels be the cause of fever, then we should always have, what but rarely exists, some degree of tenderness of the epigastrium, and a constant mucous diarrhœa; the latter symptom, particularly, should be present in every case of fever; for it may be laid down as an almost invariable rule, that a part which in a healthy state secretes a fluid, has that secretion increased when it becomes inflamed; this effect we see produced in the mucous membrane of the nose, larynx, and trachea in catarrh, for example, which consists in an inflammation of these parts; and to cite a case still more in point, we find it to be strikingly the case in dysentery. All acknowledge that the mucous membrane of the intestines is inflamed in this disease, and we see increased secretion of mucus, the result of it; the same thing happens when the lining membrane of the bladder is inflamed; as we have not, therefore, this discharge in typhus, in which inflammation of the mucous membrane of the stomach and bowels is assumed to exist, we conclude that it is not present. 3d. We cannot conceive inflammation to exist in the first

stage of fever; when the action of the heart and arteries is comparatively weak and feeble, and the blood, as a necessary consequence of this weakened action, accumulates in the venæ cavæ and their branches. 4th. This very accumulation of blood about the heart, inasmuch as it affords to it an increased quantity of its natural and legitimate stimulus, is abundantly sufficient to produce a reaction of this organ, or, in other words, to excite a fever, without bringing in a cause (i. e. inflammation of the stomach and bowels) which we cannot conceive to exist under such circumstances. And, 5th. Post mortem examinations do not prove it; they may, indeed, exhibit the evidence of inflammation in the lining membrane of the stomach and bowels, in the head, liver, lungs, spine, &c.; but this by no means proves, that the inflammation which is found to have existed in these different parts was antecedent to and produced the fever; and if we admit inflammation as a cause of fever, when found in the mucous membrane of the stomach and bowels after death, we must make the same admission when this inflammation is discovered to have existed in other parts; and, as a consequence of this, we should have as many different seats of fever as there are viscera. For these reasons we reject the doctrine of Broussais and of all others who contend that fever has "a local habitation;" and in doing so we are sustained by the opinion of the most eminent pathologists of the present day. Laennec affirms, that the organic changes which we find after death from fever, are "*evidemment posterieures a la fievre*, et ne peuvent, par consequent, etre regardes comme sa cause."*

The views of those who would limit the seat of fever to some one viscus are very unphilosophical; "they narrow an important subject which disdains such petty limits." Fever is what Dr. Hosack, one of the most distinguished physicians of this country, describes it to be, "a disease of the whole system; it appears no less in all the faculties of the mind than in all the functions of the body; it shows itself in every organ of our frame, and affects every nerve and fibre of the system; the absorbing,

* Med. Chir. Review, Jan. 1825, p. 16.

the circulating, and excreting system of vessels, are all affected by it; in a word, it is omnipresent; it has no one pathognomonic symptom, but is constituted by a concourse of symptoms, and these variously combined in the various forms that fever assumes, depending upon the cause whence it proceeds, and the condition of the body in which it occurs."*

In rejecting the doctrine of Broussais as to the local origin of fever, we entirely concur with Armstrong in the opinion, that inflammation is the product of preceding excitement, and that its particular seat is determined by the previous condition of the organ which it attacks; this condition is a debility of the organ affected.† From what we have seen we think the head is most frequently affected.

Congestive Typhus. Previous to the appearance of Dr. Armstrong's admirable work on typhus fever, this form of the disease had not attracted the attention it merited from physicians; and, in fact, we knew little or nothing of its nature. In the works of Sydenham we find the first particular notice of congestive fever; he relates the circumstances of one case only, but from the confident tone in which he spoke of what would be the effect of venesection in it, we may fairly presume that he had met with cases of the like description before. In allusion to this particular form of the fever he remarks, "that all the symptoms proceed from nature's being in a manner oppressed and overcome by the first attack of the disease, so as not to be able to raise regular symptoms adequate to the violence of the fever; all the appearances being quite irregular. For the animal economy being disordered, and in a manner destroyed, the fever is thereby depressed, which in the true natural order generally runs high. I remember (continues he) to have met with a remarkable instance of this, several years ago, in a young man I then attended; for though he seemed in a manner expiring, yet the outward parts felt so cold, that I could not per-

* Appendix to Thomas's Practice, p. 948.

† Ferriar long ago advanced this opinion; he says, that "during the increased action of the circulating system, if any part of the body be originally weak, or have been rendered infirm and irritable by preceding disease, congestion and its consequence may be expected there." Med. His. p. 150.

suade the attendants he had a fever, which could not disengage and show itself clearly, because the vessels were so full as to obstruct the motion of the blood."* After the time of Sydenham, no particular notice appears to have been taken of congestive fever. It is true, that Drs. Jackson and Clark, almost a century after, gave some imperfect account of it; but until the appearance of Armstrong's work, our notions of the nature of this fever were very obscure; he has the high merit of having clearly and ably elucidated its pathology; and of having opened a new field for medical enquiry and research. The disease, according to him, consists in an engorgement or distension of the large internal veins; and differs from his other divisions of typhus in having no stage of excitement or reaction of the heart from the first shock of the disease. We cannot do better than copy his description of the symptoms of congestion; they are portrayed in his best style. "The attacks (says he) of the most dangerous forms of the congestive typhus are generally sudden, and marked by many remarkable symptoms: an overpowering lassitude; feebleness of the lower limbs; deep pain, giddiness, a sense of weight in the encephalon; a dingy pallidness of the face; anxious breathing; damp, relaxed, or dry withered skin; and those peculiar conditions of the temperature which have been noticed above.† The pulse is low, struggling, and variable; the stomach irritable; frequently there is an inability from the first to hold up the head; and the mind is more often affected with dullness, apprehension, and confusion, than with delirium. The whole appearance of the sick impresses the attentive practitioner with the idea, that the system in general, and the brain in particular, are oppressed by some extraordinary load. Both the manner and look of the patients undergo early and great alterations; sometimes they slowly drawl out their words, or utter them in a hasty and yet imperfect mode, like people who slightly stammer when embarrassed; they not unfrequently seem as if stunned by a blow, half drunk, or lost in a reverie; and at times have the bewildered aspect of

* Wallis's Sydenham, vol. 2, p. 351. † "Shrivelled cold skin."

persons suffering under the first shock of an overwhelming misfortune. The eye is occasionally glassy and vacant, without redness; but at other times it is heavy, watery, and streaked with blood, as if from intoxication or want of sleep. At the commencement the pulse is often less altered as to frequency than might reasonably be expected, yet in general it becomes very rapid towards the close; the tongue is usually little altered in the first stage: but in the last, it is frequently rough, foul, and brown; the bowels are mostly very torpid in the beginning, and the stools procured dark and scanty; whereas, in the last stage, the bowels are generally loose, and the stools copious and involuntary. Eructations are not uncommon at all times, and the epigastric region is often much inflated. On account of the general torpor, the secretions are diminished or suppressed; and, as justly remarked by Dr. Robert Jackson, the skin is often in that peculiar state, that if blisters be applied, they either do not act at all, or so defectively, as to leave the appearance as if the part had been seared by a heated iron. Petechiæ in general appear earlier in these than in any other varieties of typhus; and in the last stage there are sometimes grangrenous spots on the extremities, oozings of blood from the mouth and nostrils, and hemorrhage from the bowels."*

"There are comparatively milder forms of congestive typhus, in some of which the patient walks about for a few days after the infection has begun to operate, and complains little, except of uneasiness of the head, loss of appetite, and languor, appearing rather paler than when in health. If strictly attended to, however, by a medical observer, a change may usually be remarked in his whole demeanor; he cannot as steadily command his attention as before, is not only restless during the day, but watchful at nights, and soon betrays an absence of mind or loss of memory. At length he becomes garrulous like a half drunken person, or talks inconsistently with his former views and character; after the lapse of a day or two the mental confusion is most obvious to every one—he begins to be unsteady in his

* Armstrong on Typhus, p. 74.

gait, and has a heavy intoxicated cast of countenance. If carefully examined at this period, his tongue will be found white; his pulse small, quick, and perhaps irregular; his breathing hurried; his bowels slow; his skin rather hot about the trunk, but coolish and damp on the extremities. If the disease be allowed to proceed, without decided interruption, the hands shortly become tremulous, and the confusion of mind passes into delirium; yet there is still a want of regular excitement, demonstrated by alternate flushing and paleness of the face, the feebleness of the pulse, the unequal state of the whole circulation, the coolness of the extremities, the partially concentrated heat of the trunk, and laxity of the skin. Aural and visual deceptions succeed, and force the patient into violent exertions, and every attempt to overpower him by coercion tends to aggravate the delirium and sink the strength. His tongue grows daily fouler and his debility greater; he begins to pick the bed clothes; and, at last, petechiæ and subsultus tendinum appear. About this period, the general turbulence sometimes unexpectedly abates, and he may become so serene and rational, as to give hopes that a favorable crisis had taken place; but the calm is most frequently deceitful, being soon followed by a universal collapse, in which death occurs, mostly without much struggling. Several cases, nearly answering to the above description, have fallen under my notice, and I have found, that if opportunely and properly encountered, they may generally be subdued; but that if overlooked or improperly treated in the commencement, they will commonly baffle the best directed measures."* There are some other modifications of congestive typhus, which it is deemed unnessary to take up time and paper to relate, because from what has been said the disease may be easily detected in all its different shapes.

Prognosis. The prognosis in congestive typhus, is to be drawn from the character of all the symptoms present in the disease. If these should become worse, if the delirium should increase, the pulse should continue to sink, the breathing to grow worse,

* Armstrong on Typhus, p. 75.

the skin become colder, and the stools be passed involuntarily, it will generally terminate fatally. If, on the contrary, the reverse of this should take place, then we may anticipate a favorable termination.

Pathology. The nature of this fever obviously consists in an accumulation of venous blood in the interior of the body. The cause of this fever, whatever it is, acting either directly or indirectly on the heart,* produces weakened action of this organ, and this latter necessarily causes an accumulation of blood in the venæ cavæ and their branches; less blood than usual being sent to the surface, paleness and shrinking of the features and surface of the body is the necessary consequence. This state of the circulation, in a greater or less degree, is common to the first stage of all fevers; the heart, however, usually reacts under the pressure of venous blood, and drives it with increased force to the exterior, when the fulless of the features and the warmth and colour of the skin return; but if, from any cause,

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*When reflecting on the modus operandi of the remote cause of fever, we have often been astonished, that all writers on the subject should have fallen into the error of overlooking its effect upon the action of the heart; and to have confined their observation to the effect (secondary and consecutive) produced on the cutaneous vessels. Thus Armstrong, in his essay on Scarlet Fever, says, "One of the most obvious operations of contagion, like that of cold, is a change of action in the cutaneous vessels, and a recoil of blood from the surface towards the centre." Now, it is perfectly obvious that the action of the cutaneous vessels is dependent on that of the heart; if the action of the latter is weak, so likewise will be that of the former—if strong or vigorous, so will be that of the cutaneous vessels. So long, therefore, as the heart preserves its action unimpaired, the cutaneous vessels must continue in the same state; but the moment that this is weakened, we then observe this "change of action in the cutaneous vessels," which Armstrong considers to be the primary and direct effect of the operation of contagion, and the first link in the series of effects which constitute fever. In consequence of this weakened action of the heart, less than the usual quantity of blood is sent to the surface, and as the skin is generally flushed, and the features full and expanded, when this action is exalted and vigorous, in fever for example, so likewise when it is weakened, we have paleness of the face and shrinking of the features and surface of the body. The phraseology of Armstrong is incorrect and calculated to mislead, when he says that contagion produces "a recoil of blood from the surface towards the centre;" for it would seem from this that the cause of fever impressed upon the contents of the cutaneous vessels a centripetal direction, which is palpably absurd. The reader will find this subject most ably discussed in a work on the practice of medicine by John E. Cooke, M. D., Professor of the Practice of Medicine in Transylvania University, a book containing more sound and original views, and a more correct pathology, than any with which we are acquainted.

the heart should fail in recovering its energy, then we have a continuance of those symptoms which characterize congestive fever. Whether, however, the cause (i. e. weakened action of the heart) assigned for this accumulation be the correct one or not, dissections demonstrate beyond doubt its existence. Armstrong says, that "In examining the bodies of some patients who had died in the most concentrated attacks of congestive fever, I have found the right side of the heart loaded with dark blood; and, in reflecting on the phenomena of all, am now inclined to believe, that their pathology is intimately concerned with the functions of the right ventricle. For (continues he) when the action of the right ventricle is diminished, and when it is overloaded by too great an accumulation of blood, it must, by consequence, occasion a remora of venous blood in distant organs; and a sufficient portion of red blood not being thereby returned to the left ventricle, its action, also, must be defective, and its blood not sufficiently oxygenized for the complete purposes of vitality."* The whole of this extract is highly valuable and important, as establishing by post mortem examination the pathology of this form of fever; but a part of it, we think, deserves particular attention, inasmuch as it may serve to explain the cause of the sudden deaths which frequently take place during the prevalence of mortal epidemics. I allude to that part in which he says, that "an accumulation of blood about the heart must produce a remora of venous blood in distant organs." Or, in other words, if the right side of the heart be full and distended with blood, this fulness must extend itself into the cavæ and their branches; following, therefore, the ramifications of the superior cavæ, we find this fulness and distension extending up the jugulars, from thence through the foramen lacerum into the larger sinuses of the dura mater; when, therefore, this accumulation is already very considerable, if, from any cause, it should suddenly increase, it may produce such pressure on the brain as to cause the person to drop down as if in apoplexy. Armstrong, indeed, says, that "the most violent forms of the con-

*Essay on Typhus, p. 84.

gestive typhus, resemble apoplexy in their symptoms, to which, indeed, they often have a near affinity in their pathology."* This is the best explanation we can offer of those sudden and occasionally instantaneous deaths which sometimes occur during the raging of violent fevers.

Treatment of Typhus Fever. As our opinion of the character and pathology of typhus fever differs so entirely from that of those who consider it a disease of debility in all its stages, so likewise will our treatment be almost diametrically opposite. We think that death not only in this, but in all fevers, rarely arises from mere debility, but from the effects of too great excitement and inflammation upon some part essential to life; our object, then, is to keep both the one and the other, if present, in a state of proper subjugation, by the steady co-operation of those remedies which are in their character strictly antiphlogistic; and we think that these, when timely and judiciously applied, will seldom fail in producing the desired effect. We do not wish to be understood as pursuing exactly the same practice in every instance; for we know that cases often occur, in which it is not necessary to bleed, or even to use very active purgation; all we wish to inculcate is, that the antiphlogistic principle of treatment is to be rigidly adhered to, whether the disease be mild or severe; leaving to the judgment of the physician the proper adaptation of the means to the *grade* of the fever. And first of

Blood-letting. The great value of this remedy in fevers has been known almost since the origin of medical science, and though amid the vicissitudes and revolutions which have been, and are still going on, it has been alternately praised as of indispensable utility, and rejected as pernicious in the extreme, its efficacy appears now to be indisputably established. Some of the ancient physicians were much in the habit of using it. Galen and Celsus thought it one of the most valuable remedies in pestilential disorders, and we are told by Prosper Alpinus that the Egyptians let blood in fevers of a putrid tendency. The Ara-

*Essay on Typus, p. 81.

bian physicians, also, frequently made use of phlebotomy in the fevers of that country. The great advocate for bleeding in more modern times is the great Sydenham; he employed it freely in many diseases, and used it in the very fever now under consideration. After contending for the inflammatory nature of the disease, and the necessity of curing it by "a suitable method and medicines," he says, that "in order to do this, I direct ten ounces of blood to be taken away from the arm, and, in effect, though the blood in this fever generally resembles pleuritic blood, yet it does not well bear repeated bleeding. But if a difficulty of breathing or a violent pain in the head in coughing, and other symptoms of this kind, show the tendency of this disease to bastard peripneumony, bleeding and purging are to be repeated, till the symptoms entirely disappear."* From this extract we learn, that in mild cases "repeated bleeding" was not borne well; but that if symptoms of local inflammation supervened, then the patient was to be bled and purged, till the symptoms were removed. These remarks are strikingly applicable to the fever as it appears at the present day: there are many cases which either do not require bleeding at all, or demand but the loss of a small quantity; such cases, of course would not "well bear repeated bleeding," but, on the other hand, cases occur, such as related by Sydenham, in which we must bleed again and again, until the symptoms are subdued.

Bleeding, in severe cases of typhus, must be used early in order to derive full benefit from the remedy. If we discover from the symptoms, that an acute inflammation of some vital viscus threatens the life of the patient, we cannot commence our operations too soon. We should draw blood *pleno rivo*, and not rest satified with a small dribbling stream; the first mode of bleeding is infinitely preferable to the last, as it effects a more speedy reduction of the pulse, and sooner brings on a state of fainting, which is much to be desired in very violent cases, as it is accompanied with vast relief to the inflamed organ.† If the

*Wallis's Sydenham, vol. 2, p. 319.

†We have a striking example of the powerful effect of bleeding ad deliquium, or until a sensation of faintness or sickness is produced, in cases of violent ophthalmia. The tunica conjunctiva, which before was as red as the presence

heart should react under this loss of blood, and the symptoms of inflammation reappear, we should, without hesitation, open the vein a second time, and bleed until the patient is relieved. It rarely happens that bleeding thus early and decisively does not entirely remove the inflammation; and though the febrile action may continue for some days after, it is easily kept in check by the use of purgatives, &c. This vigorous practice is only intended for the most exalted grades of this fever. It is every way preferable to the half-way temporising measures of timid physicians, who will sooner bleed ten ounces every day for a fortnight, than, by one or two copious bleedings in the commencement, extinguish the inflammation and sometimes the fever at once; and in this way producing much less debility, than by pursuing the opposite and much more precarious course. We are by no means to be deterred from this treatment by the appearances of great debility, which often present themselves in the commencement of this fever; these are deceptious; beneath them lurks a disease of the most speedily fatal character; which if not unmasked and detected, will soon destroy the patient. The language of Armstrong on this subject is full of wisdom. "If the topical affection (says he) has been but of short continuance, and the vigor of the constitution be merely weighed down, and not really exhausted, let him discard the fears associated with false doctrines, and promptly extract blood according to the seat and extent of the inflammation, and till local pain and general oppression be relieved."

In severe and violent cases of typhus, there is scarcely any remission, but in the milder, there is generally some little abatement of the symptoms; in such cases it is always best to use the lancet during the period of the greatest exacerbation, or acme of the fever.*

We have before said that cases of fever often occur, which

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of arterial blood could make it, will be found almost completely blanched, and the pain and tension of the eye surprisingly relieved.

* It is not a little singular that some of the ancient physicians should have recommended a different course; they thought that to open a vein in the height of the fever was death; thus Celsus says, "si vehemens febris urget, in ipso impetu ejus sanguinem mittere hominem jugulare est; expectanda ergo intermissio."

do not require the lancet at all; but that they might safely be trusted to remedies whose operation, though strictly antiphlogistic, were yet inferior in point of power to venesection, such as cathartics, abstemious regimen, &c. There are likewise many cases in which the symptoms of inflammation are so slight, that though bleeding would be proper to lower arterial excitement and thus prevent an increase of the topical affection, yet a much smaller quantity will suffice for this purpose than in the more violent instances.

We trust that in the remarks made on bleeding, we have not evinced an overweening confidence in this remedy. It is certainly one of Herculean powers, and as such ought to be employed with caution. We believe the reputation of this noble remedy has often suffered from its having been mistimed, and from its having been used in insufficient quantity. It is mistimed when it is used after inflammation has continued for several days, and when irreparable injury has been inflicted on some important part; under such circumstances, we need not say that bleeding will be unavailing; nay, worse, it will only hasten death. It is, also, often used in insufficient quantity. We are too frequently palsied by apprehensions of debilitating the patient too much; "But if our fears be so great, and we take away so small a quantity of blood, how is it possible to judge exactly what good or mischief bleeding may do? For if a disease (which requires four pounds of blood to be taken away in order to its cure, and yet but one is taken away) destroys the patient, it does not, therefore, prove destructive because bleeding was used, but because it was performed in an improper and, perhaps, unreasonable manner."* We have quoted the remarks of Botallus on bleeding in plague, because, we think, they are equally applicable to typhus and other fevers, and because they illustrate in a forcible manner the inadequacy of small bleedings to cure diseases which require copious abstractions of this fluid for their removal.

There are some symptoms of typhus which have been supposed to indicate great debility, and a putrid tendency of the fluids. These are petechiæ and a dissolved state of the blood.

*Botallus on Plague. Wallis's Syd. vol. 1, p. 130.

These symptoms have been supposed to forbid any thing like depletion, and to demand an opposite course of treatment. We do not deny that where these symptoms make their appearance late in the fever, when the constitution has been shattered by the unrestrained continuance of a mortal disease, depletory measures would be improper; but we by no means agree that they evince any thing like putridity of the fluids or even a tendency to it; this we consider to be totally incompatible with the laws of vitality. When, however, these symptoms show themselves early in the disease, we believe that they indicate a fever of unusual violence, and one which demands active and energetic measures for its relief. Sydenham thought that petechiæ were indicative of a highly inflammatory state of the system; for, when speaking of the benefits of a low diet and cooling regimen in this fever, he remarks, that "it must be indispensably enjoined, when the patient is attacked with a phrenzy, *petechiæ*, purple spots, or any other sign of violent inflammation, occasioned by an overheating regimen."* Petechiæ are very often brought out by such means as excite and inflame the system. Sydenham, to whom we always refer with pleasure, remarks in his "Essay on the rise of a new Fever," "that petechiæ frequently appear, occasioned by an unreasonable use of cordials, and a hot regimen."† De Haen, also, says, that the German physicians of his day, converted common fevers, by the use of the "regimen calidissimum," "into the petechial and miliary."‡ On the contrary, symptoms indicative of a malignant putrid character, are moderated or prevented by pursuing an antiphlogistic and cooling plan of treatment. In proof of this, we have evidence handed down to us, by the bright luminaries of the profession in times that are gone. Sydenham, after contrasting the two methods of treatment, says, "so, likewise, if I find, in the fever under consideration, that the more the patient is heated, the more he is disposed not only to phrenzy, purple spots, *petechiæ*, and the like symptoms; but farther, that the fever, by this procedure, is attended with all sorts of irregular and violent symptoms; and, on the other hand, if it appears that another patient, by treating

*Wallis's Syd. vol. 2, p. 325. †Ibid. p. 317. ‡Thes. Sist. p. 35.

him according to the method here proposed, (i. e. the antiphlogistic plan,) is quite free from such symptoms, reason shows that the latter method of practice is much the best."* De Haen, who was a strong advocate for the inflammatory nature of this fever, and, of course, for venesection, says that petechiæ never appeared in it when he had the management of it from the beginning. "Liceat me addere, (says he) quod et medici complures et ego in nosocomio, sive in vigore morborum, sive eorundem in fine, nunquam nostris in ægris, quibus a principio affueramus arbitri, miliria detrimus;"† and in the same work expresses his belief, that petechiæ would but seldom appear, even in the plague itself, if the antiphlogistic method of Botallus and Sydenham was strictly followed. Dr. Dickson, in his remarks on the contagious fever which prevailed on board the Russian fleet, after mentioning that the appearances on dissection of those who died of this fever, were "strongly illustrative of the frequency of visceral inflammation," observes "that from reflection on numerous facts, it seemed to him an inevitable conclusion, that those remedies which arrest inflammation at the commencement, prevent the graver and malignant symptoms, which characterize the last stage of such fevers; and this conclusion was amply borne out by the results of his practice, since bleeding and purging at the beginning were generally successful."‡ Rush, in his "Defence of Bloodletting," remarks, that Dr. Morton describes what he called a putrid fever, which was

* Wallis's Sydenham, vol. 2, p. 354–5.

†Although, in the above extract, the miliary eruption is mentioned as being prevented from appearing by bleeding, &c., yet as De Haen considered this eruption as nearly the same with the petechiæ, and that those means which prevented the one would also prevent the other from appearing, and that the same remedies would equally remove both when present, we have taken the liberty to quote it in this place as equally showing the effect of bleeding in preventing the appearanee of petechiæ. That De Haen considered the petechial and the miliary fever as very nearly related, and that the remarks he made upon one were equally suited to the other, is perfectly obvious from the following extract taken from the end of his section de Febre Petechiali. He says, "Multa de petechiis dicenda supersunt; maxime de iisdem tum proveniendis, antequam fiant; tum cum adsint, curandis; verum cum hoc quoque ad miliarium eruptionem pertineat, ipsaque miliarium historia eam petechiarum elucidet, atque explanet, una fidelia hunc utrumque parietem dealbabo." This quotation, we hope, will be a sufficient justification for using the above extract as we have done.

‡Armstrong on Typhus, p. 154.

epidemic and fatal, in the year 1678. Dr. Sydenham, who practised in London at the time, takes no notice of this fever. "The reason of his silence (says Rush) is obvious. By copious bleeding, he prevented the fever of that year from running on to the gangrenous state, while Dr. Morton, by neglecting to bleed, created the supposed putrid fevers which he has described." The importance of this subject will, we hope, be a sufficient apology for our having dwelt on it at some length. We consider it of great importance, in a practical point of view, to have correct notions on this subject. If we adopt the opinions of those who consider the appearance of petechiæ as at all times denoting debility, and a tendency to putrefaction in the fluids, and on this account to be treated by tonics and stimulants, we will most unquestionably do much injury. From our own experience, and from the testimony of others, we firmly believe that, when occuring early in the fever, they indicate a high grade of disease, which must be combated by active measures of treatment,* and so long as the pulse preserves its strength, we may deplete regardless of their presence.

As to the dissolved state of the blood, if it occurs early in the disease, and the other symptons do not forbid the use of the lancet, we should proceed as though it did not exist. Rush considered it as indicating a highly inflammatory state of the system. In his history of the yellow fever of 1793, he says, "I paid no regard to the dissolved state of the blood, when it appeared on the first or second day of the disease, but repeated the bleedings afterwards in every case, when the pulse continued to indicate it. It was common, (says he) to see sizy blood succeed that which was dissolved."

Cathartics. Cathartics, next to venesection, are, unquestionably, the most important remedies in this fever. Unlike bleeding, however, they are not only admissible, but, in a greater or

*Dr. Rush, in his account of the yellow fever of 1793, remarks, that "the presence of petechiæ did not deter me from repeated bloodletting, when the pulse retained its fulness or tension." After mentioning instances of its success, he says, "I find precedents in De Haen and Dr. Sydenham in favor of the practice. So far from viewing these eruptions as signs of putrefaction, I considered them as marks of the highest possible inflammatory diathesis. They disappeared in each of the above cases after bleeding."

less degree, are demanded in every grade and in almost every stage of the disease. The ancient physicians were much in the habit of employing purgatives in fever and other diseases; the number of such articles then in use was comparatively limited, and were very rough in their operation. Sydenham, in more modern times, carried the practice of purging to a very considerable extent. After his time, however, and previous to the revival of the practice of purging in typhus by Hamilton, cathartics had been in a great measure neglected in the cure of fever; particularly in that under consideration. Brown, in accordance with his belief of the asthenic character of typhus, of course, rejected them; and Cullen placed but little confidence in them, and generally substituted enemata in their stead.

We have said above that Hamilton revived the practice of purging in typhus. This assertion has not been made without due consideration, and only from a conviction that we ought, in matters of science and discovery, as in other things, to "render unto Cæsar the things that are Cæsar's." Hamilton certainly deserves great credit for shaking himself loose from the doctrines of his day; but he has no claims to originality in prescribing purgatives in typhus fever. If we refer to Sydenham, we shall find that he used them freely; and in the very disease now under consideration placed his chief reliance on bleeding and purging for its cure. In his "Essay on the rise of a new Fever," (which is, as was before mentioned, the typhus of the present day,) after recommending the use of purgatives and bleeding, he remarks that "the method just recommended is the best I ever tried in curing this fever; and if it fails of effectually removing it, at least brings it to intermit, and then it always yields to the bark. But as purging, as it is here directed in order to cure this fever, may perhaps seem detrimental to some persons, I assert from experience, that nothing cools so much and so surely as purging after bleeding." After assigning some reasons for their beneficial effects, he says, "For these reasons, therefore, I hope I may assert, on good grounds, that *the method of cure above delivered, which consists in bleeding and purging, is the*

most effectual one to conquer most kinds of fevers;"* and in other passages, he enforces the same practice. It will be seen from the above extracts, that nearly a century anterior to the time of Hamilton, Sydenham prescribed purgatives in this disease from the beginning, and recommended them, conjoined with venesection, as the best cure for most kinds of fever.†

Hamilton, it seems, was led to the employment of purgatives by mere accident: he says, "At the time I was appointed physician to the Royal Infirmary, the cure of typhus was thought to consist chiefly in the removal of atony and spasm of the vessels of the surface of the body. For this purpose, among other remedies weak antimonial wine and nauseating medicines were freely given. The state of the stomach and bowels, after the exhibition of an emetic and purgative on the first approach of the attack, was little regarded in the after periods of the fever. An occasional stool was procured by a mild glyster, while a purgative medicine was given with extreme caution. Apprehensions were entertained that the operation of a purgative would rivet the spasm of the extreme vessels, and increase debility, one of the supposed direct causes of death in fever. These apprehensions (continues he) may still bias the practice of many, as they certainly did bias mine, for a long time." An unusually malignant typhus fever, however, breaking out in Edinburgh, in the summer of 1779, and being unable to cure it by the mild antimonials, Hamilton was induced to resort to the *calx antimonii nitrata*. "This antimonial remedy (says he) was not ineffectual; but I remarked that it was beneficial only when it moved the belly. The stools were black and fœtid, and in general co-

*Wallis's Sydenham, vol. 2, p. 344–5.

†It has, we think, been too much the custom to overlook the practice of Sydenham, and to consider his great excellence to consist in his unrivalled descriptions of acute diseases. We, however, look upon his writings in a very different light: he certainly deserves great credit for his accurate account of diseases, but we by no means consider it as his highest praise. The improvement he made in the treatment of smallpox is enough to immortalize any one man, and his mode of curing fevers and the phlegmasiæ has been but little improved on even at the present day. There is one striking instance in which his claims to originality of treatment are almost entirely overlooked, and all the credit assigned to Hamilton. I allude to the method of curing St. Vitus's Dance. He used identically the same routine of practice in this disease which is followed at the present day upon the authority of Hamilton. Any one may satisfy himself of the truth of this by consulting his works.

pious. On the discharge of these the low delirium, tremors, floccitatio, and subsultus tendinum, which had prevailed, abated in some cases; the tongue, which had been dry and furred, became moister and cleaner, and a feeble creeping pulse acquired a firmer beat. Reflecting afterwards on these circumstances, it occurred to me, as the purgative effect appeared to have been the useful one, that any purgative medicine might be substituted for the calx antimonii nitrata, and that by this substitution, the unnecessary debilitation of an exhausted patient, by vomiting and sweating, might be avoided."

"More extended experience confirmed these conjectures; and I was gradually encouraged to employ purgative medicines early in typhus, and to repeat them in the course of the disease; and after having long and strictly directed my attention to this point of practice, I am now thoroughly persuaded that the full and regular evacuation of the bowels relieves the oppression of the stomach, and mitigates the other symptoms of fever."*

These extracts are interesting and useful, as they give the experience of an accomplished and practical physician; and as showing the gradual dawning of light on a mind clouded and fettered by the scholastic doctrines of the day.

Huxham, however, anterior to the time of Hamilton, had remarked the great and surprising relief experienced from the free discharge of dark bilious matter from the bowels. In his essay on "malignant putrid fever," he remarks, that "unquestionably the bilious principle is too greatly predominant in all putrid, malignant, and petechial fevers. The gall bladder, and biliary ducts, are always found full of black or green bile in those that die of pestilential diseases, and so is the stomach, duodenum, &c. Now, (continues he,) if this putrid bile is not carried off, it grows more and more corrupt, and causes vast anxiety, sickness at stomach, pain, &c.; and, being reabsorbed into the blood, creates infinite evils, greatly irritates the *genus nervosum*, destroys the *crasis* of the blood, and turns the lymph

* Hamilton on Purgatives, p. 29.

into a corrosive ichor. When, therefore, there are signs of its being redundant, it should be forthwith discharged by vomit or stool, as nature points out. I have many times, with the greatest pleasure, in these putrid fevers, seen an amazing change for the better immediately succeed a fit of vomiting, and a stool or two, when an inexpressible anxiety, load on the *præcordia*, perpetual sickness, *eructation*, and *singultus*, had preceded. The extreme foulness of the tongue, and load at the stomach, with a loathsome bitter taste, and a horrible offensive stinking breath, and eructations, show the condition of the stomach; and the abominable fœtid, black, bilious stools, the necessity and advantage of that discharge."* This extract from Huxham, though a long one, is, we conceive, highly valuable, inasmuch as it informs us of the state of the gall bladder, biliary ducts, stomach, and duodenum, after death from this fever, and mentions the effect which a retention and reabsorption of this dark bile has upon the genus nervosum. The first points out the indispensable necessity there is for cathartics, and the last teaches us that many of those nervous symptoms incident to the disease, and which have been supposed to proceed from debility, and treated as such, are to be attributed to a retention of this black bile, and can only be relieved by its evacuation.

Even among those, however, of the present day, who agree upon the utility of free purgation in this fever, there is much diversity of opinion as to the kind of cathartics which are best adapted for this purpose. While some proscribe the use of calomel and all other active purges, and rely on laxatives almost entirely to effect the evacuation of the bowels, there are others who look on it as the sheet anchor of hope, not only in this, but in most other fevers, and prescribe it with a degree of boldness and daring perfectly astonishing to those who are accustomed to administer it only in five or ten grain doses. Either practice may be carried to extremes, but of the two, in severe cases of fever, we should greatly prefer the bold exhibition of active remedies to the milk and water practice of those who would

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* Huxham on Fevers, p. 94.

altogether exclude such means from the treatment of fever. We have always been the steady and constant friend of the use of calomel in fever; but in ordinary cases we believe its moderate to be just as beneficial as its extravagant exhibition, with this advantage, that there is less risk of producing that painful, disgusting, and sometimes fatal affection of the mouth, a salivation. This Hercules of the materia medica is not to be rashly dealt with, without often leaving behind the most lamentable and distressing consequences of its mal-administration. When judiciously used, however, it may truly be said to be an invaluable remedy; there is no article in the whole materia medica which so effectually discharges that dark bilious matter, which affords so much relief in fever. It is customary to combine it with some other cathartic, and we know of none better than jalap; ten grains of each for an adult, administered every two hours, until the bowels are freely evacuated, constitutes as effectual a purge as can be used. This combination was much employed by Dr. Rush in the yellow fever of 1793, and it was then called a dose for a horse; what would they who so called it have thought of sixty grains of calomel as a common dose? Jalap, however, to some stomachs is so exceedingly nauseating and offensive, that we are obliged to substitute something in its stead; this may be done by adding the same amount of scammony to the calomel, and administering it in the same way. Scammony has hitherto been but little used on account of its supposed drastic and harsh operation, but from repeated experience we can declare that it is less so than jalap, and is equally effectual in evacuating dark bilious matter. Another mode that we have found very effectual in evacuating the bowels, and lowering the action of the heart and arteries, is to administer calomel alone, and sometime afterwards to give a strong infusion of senna, with either Epsom or Glauber salts. This generally operates most copiously, the discharges being very thin; it is, therefore, only suited to the beginning of the disease, when the strength is but little impaired, and to those of strong constitutions; it is very apt to prostrate the thin and delicate too much. The cathartics are to be repeated day after day, until

the fever is subdued; when, however, it has continued for some time, and the patient is much weakened, we generally combine aloes with the calomel and jalap, to render the discharges more consistent. The following is the prescription in general use:

Calomel, 3ss.
Jalap, 3i.
Aloes, 3i.
} M. D. in pil: xxx.

Four or five of these pills given and repeated according to circumstances, will generally operate freely enough. The discharges produced by this combination are much more consistent, and, of course, less debilitating, than those by calomel and jalap alone. If at any time during the exhibition of these pills, the mouth should become sore, the calomel must for a while be discontinued, and the same quantity of aloes and jalap be given alone; this combination is the very best substitute for calomel that we are acquainted with. When the patient is fairly convalescent, it is necessary to pay much attention to his bowels, if they should not be sufficiently open; then two or three of the above mentioned pills ought to be given. There are, however, other articles, which, for this latter purpose, answer very well, such as castor oil, magnesia, &c.

The above practice is suited to those cases in which the bowels are easily operated on. Cases, however, of great torpor sometimes occur, in which it is impossible to obtain an operation by the usual doses. In such, it is idle to limit ourselves to a certain number of grains; we must exhibit cathartics in doses *sufficiently large to purge*, for without this the patient will die; we believe that the safety of the patient in fever, depends upon a continued evacuation of bile from the bowels, and keeping the action of the heart and arteries in proper control by the other antiphlogistic means; we must then judge of the necessity for an increase of the dose, by the effect produced, and not by the number of grains given. Fortunately cases of such extreme torpor of the bowels are rare; but when they do occur, they are only to be managed by the most bold and intrepid exhibition of medicines; in the language of Hippocrates, "ad extremos morbos, extrema remedia sunt exquisite optima." The next remedy in point of importance is

Cold Water. Cold water was much used, both internally and externally in fever, by the Greek and Roman physicians, some of whom relied upon it exclusively as a means of cure. We find mentioned, by both Celsus and Galen, that Petro, a physician who flourished a considerable time after the death of Hippocrates, attempted to cure fever by copious draughts of cold water. It appears from the description of his method, given by Celsus, that in cases of fever, he first caused the patient to be covered with a good deal of clothing, in order that he might excite in him great heat and thirst, and then commenced the profuse administration of cold water.*

The use of cold bath was introduced at Rome during the reign of what was called the methodic sect. From its having been first successfully used in the case of the emperor Augustus, whose complaint had baffled all the usual applications, it acquired great reputation, and was subsequently used with considerable boldness by some of the Greek and Roman physicians. The Arabian physicians, also, employed it much, both internally and externally, in pestilential diseases. The practice, however, was, in progress of time, lost. During the last century its use was revived by doctors Jackson and Wright.† The following is Dr. Jackson's account of the circumstances which

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* Siquidem apud antiquos quoque ante Hierophilum at Erasistratum, maxime post Hippocratem fuit Petro quidem, qui frebricitantem hominem ubi acceperat, multis vestimentis operiebat, ut simul calorem ingentem sitimque excitant. Deinde ubi paulum remitti ceperat febris, aquam frigidum potui dabat." Celsus Lib. iii, c. g.

† We find it asserted in vol. i. p. 89 of Johnson's valuable work on Tropical Climates, that "it has been practised in the Bengal remittent, time immemorial, among the natives themselves, many a century before a Jackson, a Wright, or a Currie thought or wrote on the subject" To prove this, he introduces an extract from the Oriental Field Sports, by captain Williamson, which we will copy here. "We must however, (says captain Williamson,) do the natives the justice to allow, that the refrigerating principle lately adopted by some of our leading physicians, owes its origin solely to the ancient practice of the Brahmins, or Hindoo priests, of whom the generality affect to be deeply skilled in pharmacy." And he then adds, "I believe that, if taken in time, few fevers would be found to degenerate into typhus, and that very seldom any determination towards the liver, in acute cases, would occur, were the refrigerating course to be adopted. Often (says he) have I known my servants, when attacked with fever, to drink cold water in abundance, and to apply wetted cloths to their head, with great success. The former has generally lowered the pulse considerably by throwing out a strong perspiration, while the latter has given immediate local relief."

led him to the employment of cold bathing. He says, that "the first hints of this practice were accidental, and arose from a conversation I had with the master of the vessel in which I went passenger to the West Indies. This person commanded a transport in the war of 1756, and was present at the seige of Havana. As he was talking one day of the state of the fleet, he mentioned accidentally, that some men were sent aboard of his ship ill of fever; several of whom, he observed, jumped into the sea during the delirium which attended the paroxysms of the disease. Some of them, as might be expected, were drowned; but the most part of those who were recovered from the waves appeared to be greatly benefited by the ducking. The fact, which, from the veracity of the man, I thought I could depend upon, struck me strongly, and I resolved in my own mind, to bring it to the test of experiment as soon as an opportunity should offer. Neither was it long after my arrival in Jamaica, that I had occasion to visit a sailor whose situation seemed to justify such a trial. The poor man was aboard of a ship, which lay at anchor about a mile from the shore. He had been ill two days; the delirium ran high; his eyes were red and inflamed; his respiration was hurried; he was anxious and restless in a high degree, while, together with those marks of excitement, he was occasionally languid and disposed to faint. His skin being dirty, furnished an ostensible excuse for trying this remedy. But it was previously thought proper to draw some blood from the arm; which being done, some buckets of salt water were dashed on the shoulders. He was now laid in bed; a copious sweat ensued, succeeded by a distinct remission and total change in the nature of the symptoms." After relating some other instances of its astonishing efficacy, he says. "I shall only add, that I have tried the remedy, in various situations, always with safety, generally with astonishing success; so that I cannot forbear recommending it even at an early period, in the fevers of the West Indies."* In the year 1777, Dr. William Wright, of Jamaica, published an account of the salutary effect

* Jackson on Fevers.

of ablution with cold water upon himself and others, while laboring under severe fever. This narrative of Dr. Wright excited much interest and drew the attention of many distinguished men to the subject. Among others, Dr. Jas. Currie, of Liverpool, entered into it with much zeal, and soon after enjoyed extensive opportunities of testing its efficacy. On the 9th of December, a contagious fever made its appearance in the Liverpool Infirmary, and spread itself rapidly. In this fever he, for the first time, commenced the use of cold ablution, and out of seven cases treated by it not one died; encouraged by his success in this first trial of the efficacy of cold water, he continued to use it freely, both in hospital and private practice, with extraordinary success; and some time after published his admirable work on the use of cold water, in which are contained the results of his extensive experience with it, and the rules which should guide us in its use.

No one can peruse this volume without being convinced, that cold water, when properly applied, is a most important remedy in fever. Its utility is not confined to typhus; it is equally serviceable in all fevers attended with increase of heat and arterial action. Its effect upon the pulse is astonishing in many cases. We have often known the mere bathing the arms and hands of a febrile patient reduce the action of the pulse ten or fifteen beats in the minute; and if this partial application of cold water has such an effect on the action of the heart, how much greater must be the effect of a cold bath? We have many instances on record of its calming at once the most furious delirium; persons in such a situation have often jumped overboard a vessel into the sea, and been taken up perfectly calm and rational, and with an almost complete extinguishment of the fever. With the many strong instances recorded in various works, of its remarkable efficacy in curing fever, it is justly a matter of surprize, that physicians so seldom call its great powers into requisition. It exercises a more immediate control over the action of the heart than bloodletting. Dr. Currie mentions a striking instance of the effect of cool air in reducing the pulse. "In the month of May, 1801, (says he,) I was desired to visit a patient ill of fever

in Sparling street. I found him in the tenth or eleventh day of the disease, delirious and restless; the surface of the body dry, and his heat 104° of Fahrenheit. The room was close, and I desired the only window in it to be opened. The wind from the northwest blew directly into this window, and the bed being situated between it and the chimney, a pretty brisk stream of air passed over it. The patient had just thrown off a considerable part of his bed clothes, and was exposed naked to the breeze. I sat by him with my finger on his pulse, watching the effect. In a little time the pulse fell from 120 to 114 in the minute; he became more tranquil, and soon afterwards he sank into a quiet sleep, in which he remained when the water for affusion was prepared; of course we did not disturb him;"* he remained exposed to this cold air until morning, when his pulse was found to be about 100, and his heat 101.°

Dr. Rush, in his history of the yellow fever of 1793, relates a similar case to the above. "Dr. Griffits (says he) furnished a remarkable instance of the influence of cool air on the fever. Upon my visiting him, on the morning of the eighth of October, I found his pulse so full and tense as to indicate bleeding; but after sitting a few minutes by his bed side, I perceived that the windows of his room had been shut in the night by his nurse, on account of the coldness of the night air. I desired that they might be opened. In ten minutes afterwards the doctor's pulse became so much slower and weaker that I advised the postponement of the bleeding, and recommended a purge instead of it;" and in the next page he remarks, that "cold water when applied to the feet, as certainly reduces the pulse in force and frequency, as warm water applied in the same way produces contrary effects upon it. In an experiment which was made at my request, by one of my pupils, by placing his feet in cold pump water for a few minutes, the pulse was reduced twenty-four strokes in a minute, and became so small as hardly to be perceptible." These facts teach us, that it is the cold alone which is serviceable, since it has the same effect whether applied in the

*Medical Reports, p. 259.

shape of water or air; and they also teach us the powerful influence it has over the action of the heart, and the great value of its proper use in the cure of fever.

Cold water may be used either internally or externally, or in both ways. It has been already mentioned that Petro relied entirely on copious drenching with cold water for the cure of fever; and about the middle of the last century (1752) a treatise was published on the internal use of cold water, under the title of "Febrifugum Magnum," by Dr. Hancock, London. The most effectual way, however, to derive full benefit from the action of cold, is to use it in both ways; the same principle which would induce us to resort to affusion, would also suggest its internal administration. The latter is generally so consonant with the patient's inclinations, that there is usually no difficulty in its application, and, therefore, it should always be recommended; the affusion, however, is looked upon by society as disagreeable if not hazardous, and hence, in proposing such a remedy, we have generally to encounter much prejudice. In severe cases, peremptorily requiring the use of active means, we should disregard this feeling; but if otherwise, we may be satisfied with its partial external application. We can always have the feet, arms, and hands bathed in cold water, and cloths dipped in the same applied to the stomach and head. This affords very great relief, and ought never to be omitted.

But cold water is by no means to be used indiscriminately in every case of fever; neither is it to be used in all stages of any fever; the rules which Currie has laid down on this subject are excellent, and cannot be followed too closely. If we obey strictly his directions, we will always be prevented from misapplying or doing injury by its use. He gives separate rules for the external and internal use of cold water; but as its effect, except in degree, is the same when used either way, so one set of rules will answer as a guide for both. His first general rule is, that "It may be used (either internally or externally) *when there is no sense of chilliness present, when the heat of the surface is steadily above what is natural, and when there is no general or profuse perspiration.*"

We will now give the substance of the particular rules he has

laid down on this subject. 1st. "Cold water is not to be used either internally or externally in the cold stage of the paroxysm of fever, however urgent the thirst;" taken at such times, it increases the chilliness and produces great weakness of the pulse, and if used to any great extent might cause the death of the patient. 2d. When the hot stage is fairly formed, and the surface is dry and burning, cold water may be used both ways with the utmost freedom; frequent draughts of cold liquid, and its external application, under such circumstances, are highly grateful; they diminish very much the heat of the body, and lessen considerably the volume and frequency of the pulse. 3d. "It is also necessary to abstain from the use of cold water when the body is under profuse perspiration, and this caution is more important in proportion to the continuance of this perspiration."

We will finish what we have to say on the use of cold water, by a few remarks on its *modus operandi* in curing fever. And in the very commencement we cannot help expressing our surprise, that cold should ever have been considered as a stimulant. If it be such, then is Currie's general rule for the use of cold water an absurdity; for a stimulant would do no injury during the presence of a chill, and would be obviously prejudicial in the heat and exacerbation of fever. However wild and imaginative Brown may have been in many of his notions, his opinion that cold can never act as a stimulant is unquestionably correct. It is so plain a matter that it is unnecessary to argue much about it. We will only state a few positions, the truth of which none can deny. In the first place, if we take a person in health, and immerse him in cold water, and then feel his pulse, we will invariably find it small and weak, and it will continue so as long as he remains in the water. 2d. If we immerse a person labouring under a chill, when the pulse is already weak, it will be rendered much weaker, and has sometimes been extinguished. 3d. Currie relates many cases of fever, in which the application of cold greatly reduced the action of the heart; in one case, in a very short time, the pulse was diminished eight beats in the minute; and in the case mentioned by Rush, the

pulse was lessened twenty-four strokes in a minute, by the immersion of the feet in pump water. These facts unquestionably prove, that cold has none of the properties of a stimulant, since it weakens the pulse in a state of health, weakens it still more when already weak, as in a chill, and lessens very much its volume and frequency when in a state of excitement, as in fever. "But," say the advocates for the stimulant property of cold, "if you take a person out of the cold bath, his pulse rises, his skin becomes warm, and his cheeks flushed, and hence we argue that it stimulates the system." In answer to this, we remark, that one might just as well contend that brandy was sedative in its character, because a man that had been intoxicated by it was rendered sober by its withdrawal, as to argue that cold is a stimulant, because the heart reacted when removed from the sphere of its influence. So long as we keep a person in the cold bath, so long will his pulse continue small and weak; but if we remove him from the bath, then his pulse rises; and from what cause? Surely not from the cold, because, having been withdrawn, it has, of course, ceased to operate; but from the increased accumulation of blood about the right side of the heart, which is the necessary and inevitable result of its weakened action, which last is produced by the cold application. We have then a sufficient cause for this increased action of the heart and flushing of the cheeks, in the presence of an unusual quantity of the heart's natural stimulus, without attributing it to cold, which, as we said above, has a directly contrary effect, so long as its application is kept up. These are some of our reasons for considering cold not to be a stimulant. The same facts or reasons, however, which prove it not to be stimulating in its properties, prove it to be sedative in its character. It is from its operating in this way, that we can readily account for its extraordinary effect in fever. It is by diminishing suddenly and greatly the very action of the heart, that a cold bath will calm, almost in an instant, the most raging and unmanageable delirium.

[TO BE CONCLUDED IN THE NEXT NUMBER.]

REVIEW.

"*A Treatise of Pathology and Therapeutics.* By JOHN ESTEN COOKE, M. D., Professor of the Theory and Practice of Medicine in Transylvania University. In three volumes. Lexington, Kentucky. 1828."

[CONTINUED FROM PAGE 282.]

WE proceed with our review of this work to the second volume, which, as it is directly practical, will be found more interesting than the first. It is a full development of the author's peculiar practice in a variety of disorders, and contains many new and important additions to the therapeutic branch of medical science. He presents the diseases of which he treats in an aspect different from the one in which they have generally been seen, and applies to them a method of treatment, in some cases, entirely novel.

Having shown in the preceding chapters, that the tendency of all the remote causes of fever is to produce weakened action of the heart, the author proceeds in the 18th to inquire what is the effect of this weakened action;—among which he mentions, a diminution of the quantity of blood sent into the arteries, whence weakness of the pulse, paleness and coldness of the surface, &c.—an accumulation of blood in the vena cava &c.—This accumulation extends to all the branches of the vena cava not supported by valves or muscular action;—principally, the internal iliac veins and their branches, the vena portæ and its branches, the renal, subclavian, and internal and external jugular veins. This chapter contains an accurate description of the vena cava, and exhibits clearly the points where congestion is most apt to occur.

In chapter 19 the effects of an accumulation of blood in the vena cava are considered. These are, pulsation of the vena

cava in the abdomen;—in the breast, occasioning the feelings called palpitations;—a beating in the head;—shortness of breath, enlargement of the liver, serous effusions, hæmorrhages, increased and decreased secretions, &c.

With the 20th chapter the author introduces therapeutics, which most readers no doubt think too much in the rear. The amount of matter passed over on this point, is truly great; but it is relevant, directly in point, and has an important bearing upon the pathology of diseases. This chapter is taken up with "a general view of the treatment of the preceding morbid affections.

"The effects of accumulation of blood in the venous cavity variously combined, and sometimes one and sometimes another more prominent than the rest, constitute most of the diseases to which man is liable.

In order to cure any of these, that is, to remove the effects, it is obviously necessary to remove all the causes that may still be in operation. Hence it is necessary to have in view the following objects;

I. To remove the remote causes which may still be operating on the heart, weakening its action;

II. To excite and support the action of the heart, if increased action have not come on; or in the intervals of increased action, if it have; or if it have subsided into habitual weakened action;

III. To reduce the quantity of blood accumulated in the venous cavity, in consequence of the weakened action of the heart;

IV. To reduce the action of the heart, produced by the press of blood from the venous cavity, if it exist."—Vol. ii. p. 122.

If the remote cause be the use of spirituous liquors, &c. they must be laid aside; if "poor fare",—which, however, as the author justly remarks, is rarely the cause of indisposition in our country, and especially the western portion of it—light nourishing diet must be advised; if excessive fatigue, intense application of mind, the dense gas arising from putrefying vegetable matter, they must severally be avoided.

"In order to accomplish the second object, we must consider weakened action of the heart as suddenly produced, and calling for immediate relief; as occurring periodically, and calling for efforts to prevent its return, or to moderate its violence; or as habitual, in consequence of having been gradually produced, and requiring the application of remedies more durable in their action.

When the action of the heart is suddenly and greatly reduced, efforts should be made to raise it by warm drinks, warm applications to the surface, and by emetics."—pp. 124–5.

No time is to be lost in procuring teas; warm water is equally effectual. In cases of extremity, warm toddy must take the place of both, and warm applications must be made to the surface, which are improved by the addition of salt, or muriatic acid to the bath.

"The third object is to lessen the quantity of blood in the venous cavity. This is to be effected by evacuations from the liver; and by bleeding, particularly in the jugular veins.

The medicines which most effectually accomplish the former, belong to the class of cathartics; but many of them are very far from answering the purpose. The object being to evacuate from the venous cavity through the biliary ducts, the test of success is the colour and consistence of the discharges. If they are yellow, green, dark, or black, the purpose is effected, because we know that these colours are given to the contents of the intestines by the fluids discharged from the liver. These dark discharges are sometimes perfectly mild, and inodorous.

From much observation and many experiments, I consider the medicines in common use as standing in the following order, in reference to their fitness to effect this object, the most effectual being first named; viz. calomel, aloes, rhubarb, jalap, scammony, colocynth, senna, and calcined magnesia. Salts of all kinds, castor oil, and gamboge are little adapted to this end. In some cases either of them will in a degree answer the purpose; but in general, particularly after free purging has been once or twice effected, oil only discharges what may happen to be in the intestinal canal; and gamboge and salts produce an immense watery discharge, at first coloured by the contents of the bowels, but if continued, transparent.

Calomel is slow in operating, often requiring twelve hours to effect a passage; it does not operate frequently, but the discharges are large. It gripes little or none in general, and is the most effectual medicine known, in producing consistent viscid discharges of various colours. Its action is more durable than that produced by other cathartics; accordingly it has been long observed that calomel prevents the constipation which follows the use of other medicines.

Aloes resembles calomel in its operation more than any other vegetable cathartic. From actual experiment I am convinced it is the best substitute for calomel, with reference to the production of consistent discharges from the bowels. It resembles it in being slow, in not operating more than once or twice freely, and in griping very little in general; and to it also is justly attributed the property of a durable action on the bowels.

A very strange idea prevails that the action of aloes is limited to the lower part of the intestines. The idea must have have proceeded from the usual small dose operating only once in general, and from that discharge being merely soft. The same however is observable on giving any cathartic in such dose as to operate slowly, and but once. Calomel,

rhubarb, and even jalap, sometimes act in the same manner. Aloes however, as well as the rest of these, in large doses operates more freely, producing several loose bilious passages. I have given forty grains of calomel for several days in succession, and forty grains of aloes for as many more to the same patient, and found them act very much alike; the aloes producing such passages as those above mentioned. That aloes acts on the rectum alone is impossible, because it is very soluble in however dry a state it may be; and therefore must be in a situation to act, immediately on being taken into the stomach: that it is brought into as fluid a state in the stomach as other cathartics, is evident from the appearance of the medicines when they happen to be thrown up, a short time after being swallowed. The discharges produced by aloes are also more purely bilious and acrid than those produced by almost any other medicine, perhaps by any other; which certainly shows it operates on the upper part of the intestines as well as the lower.

Rhubarb is next, of the vegetable cathartics, to aloes in these properties. In doses of five or ten grains it acts very much like the latter; but it is not quite so certain, durable, or effectual in producing tenacious or consistent discharges. In large doses it operates more freely than aloes.

Jalap in small doses is less certain to operate than rhubarb. In large doses it is more active, the discharges being larger and more liquid, but still of a cast decidedly bilious. Its action is quicker, and less durable. It nauseates very much, and leaves the stomach more out of order, than any of the medicines mentioned as adapted to the end in view.

Scammony is more active than jalap, but much like it in its mode of operation; the chief difference being that the scammony is nearly twice as active, that it does not nauseate so much, and does not leave the stomach disordered. In consequence of the great complaints made of these disagreeable effects of jalap, trial was made of scammony, and it was found an excellent substitute. It is so mild in its operation, and so little nauseating, that those accustomed to the use of it, are unwilling to do without it. It is the scammony from Aleppo that is here intended. It is very hard, dry, heavy, and of an iron grey colour.

Colocynth I have made little use of, but conclude from a few experiments, that it produces about the same effect as twice the quantity of jalap, with more nausea and griping.

The infusion of senna is a very active cathartic. The discharges produced by it are much like those from jalap. It however gripes much more than that medicine.

The calcined magnesia produces discharges very similar to those from the senna, but in small doses less serous.

Of these medicines calomel being the most effectual in producing discharges from the liver, and being also tasteless, the dose small, and weighty, whereby it is retained when other medicines are rejected, would be used to the exclusion of all others but for one evil consequence of its free use, ptyalism. This is so dreaded, and is really so dreadful,

that it becomes necessary to limit the use of this medicine. Its value is however so great, that it will always continue to be used as freely as the apprehension of salivating will permit; and in many cases that danger should be and will be disregarded even by the patient.

The necessity of limiting the use of calomel leads to the employment of different cathartics in conjunction with it.

After long and very close attention to this subject, I am led to believe that the best combination that can be devised to produce free, yet consistent evacuations from the liver, is the following: viz. equal quantities of rhubarb, aloes, and calomel, made into a mass for pills with water alone. Six grains of the mass, besides the water, viz. two grains of each of the medicines named, form a pill of good size. In general from two to four such pills operate in eight or ten hours, without nauseating, or griping, and produce about three abundant, loose, passages. One pill will sometimes produce this effect; two will frequently do it."—pp. 130–4.

For the various combinations of these articles required by different constitutions, and the varying shades of disease, the reader is referred to the treatise itself. Much stress is laid upon the character of the discharges, as they are supposed to denote the beneficial or injurious operation of the remedy. To aid the purgatives in reducing the quantity of blood in the vena cava, blood-letting is advised; but the operator should be a man of nice discrimination, and should understand well the condition of his patient, otherwise this operation might be attended with the most unhappy consequences.

"The fourth object is to reduce the action of the heart, if it exist. This is effected by bleeding, by purging, and by cold applications. Great discharges from the skin and kidneys, also reduce the action of the heart; but we cannot rely on producing these evacuations by any known means. It is however completely in our power at once to control increased action by the means above mentioned.

The cathartics used for the purpose of accomplishing the third object may be made to effect the last. The dose may be increased, or repeated, so as to evacuate to any desired extent. It is however proper to time the doses of these medicines, in such a manner as to derive from them all possible benefit. They should be given when the action is low so as to raise it, and at the same time to operate on the bowels during the rise of the action, and keep it from running too high. Used thus, these cathartics are so effectual that no other means of reducing the increased action in most cases is necessary.

There are cases however in which the action is so great, that this evacuation is utterly insufficient to reduce it; but it is completely in the power of the practitioner to effect the reduction by bleeding."—p. 158.

"Another most effectual and perfectly safe means of reducing increased action of the heart, is the affusion of cold water. Currie's Reports of the effects of this practice, in all parts of the world, fully establish its safety and its efficacy. The only danger is in the abuse of this remedy, or using it at improper periods. It is intended to reduce increased action: if it be continued till the action is reduced very low it is abused. The cold stage is an improper time to use the remedy, as it might readily extinguish the action of the heart when already low. The danger of this abuse of the remedy was soon discovered and pointed out by Currie."—p. 161.

In chapter 21, we have "the application of the principles advanced to sundry diseases;" which we shall pass over without any quotations, and hasten on to the next chapter, which treats of apoplexy.

Depending upon an accumulation of blood in the venous cavity, as the remote causes of this disease, as well as its symptoms lead us to believe it does, the method of cure is the one which has been already indicated; to-wit, bleeding and cathartics.

Epilepsy is treated of in chapter 23. After enumerating its remote causes, the author proceeds:

"All these remote causes produce directly or indirectly weakened action of the heart, and consequently accumulation of blood in the venous cavity. The proof of the presence of this state is seen in the pale countenance of persons subject to epilepsy, and in the presence of other effects of the same accumulation.

The convulsion itself is an effect of this fulness of blood; but there are many other effects of the same."—p. 196.

"In the cases which have occurred in my practice, the patients have been pale or sallow, the liver has been disordered, the discharges green or dark. One of them, besides these symptoms, was frequently disturbed in sleep and started up, and was after sitting up affected with stupor. Another had palpitation and pain in the head.

In another case the patient had the following symptoms; pain in the right side, costiveness, indifferent appetite, flatulence, weak pulse, cool skin and extremities, vertigo, passages very dark, and dull pain in the epigastrium."—p. 197-8.

"All these symptoms are effects of accumulation of blood in the venous cavity, as has been shown already."—p. 198.

"*Method of Cure.* In the treatment of this disease we must not consider it as consisting of the convulsions alone; but likewise of all the symptoms which precede them. When the convulsions occur they require

immediate attention; but efforts must be made in the intervals likewise, if we would cure the patient.

We must therefore consider the treatment in the fit, and in the intervals.

In the fit the main dependance is on bleeding if the pulse be strong; and this is almost uniformly the case."—p. 202.

"Besides the bleeding, it is proper to attempt giving a cathartic. Calomel is most proper on every account. It is small in bulk, weighty, and therefore cannot easily be rejected, but on the contrary will by the mere agitation make its way down the throat, if the patient lie on his back. It is slow but very sure; the dose should be large.

Injections should also be used if the agitations will admit."—p. 203.

"The interval between the fits is the time when strenuous efforts should be made to cure the disease.

If the pulse be strong, the patient should be bled. This however is arely the case, except an occasional increase of the action of the heart rarising out of occasional exposure to the remote causes.

If the pulse be weak it is dangerous to bleed."—p. 203.

"Whether the pulse be weak or strong, purging in the mode already stated is necessary; and if the pulse be weak it is the principal dependance. It is unnecessary to be more particular here, as the subject has been already fully discussed."—p. 204.

The author has a few words on *Chorea*, in this chapter; the only case of which that ever fell under his notice, he treated successfully by bleeding and purgatives.

On *Dyspepsia* the author has, as he ought, a long article. This occupies chapter 24. After the elaborate treatise on indigestion, by Sir Wilson Philip, and the more simple, but equally rational monograph of Dr. Johnson, one might suppose that nothing further need be written on this subject. Nevertheless, we are persuaded that there is more sound, efficient practice—practice that will eradicate the disease, and restore the stomach, "weak as blotting paper," to its former vigour—in this chapter, than in both of those works. He believes that it is produced by an accumulation of blood in the venous cavity, the result of weakened action of the heart, for which he recommends, as in the preceding diseases, a combination of calomel, rhubarb and aloes. There are cases of indigestion, he believes, depending upon excessive repletion, when the pulse is full and active, and the face red, which require nothing but a diminution in the amount of aliment. But when the countenance is

pale or sallow, the pulse feeble, and the bowels habitually disordered, exhibiting a general derangement of the venous cavity, in his practice he has found the above plan of treatment alone successful. The cases which he gives in illustration of this practice are forcible and interesting.

Nephritis occupies the 25th chapter. All the symptoms of this disease denote an accumulation of blood in the venous cavity, particularly that branch of it seated in the kidneys. On its treatment the following are the author's words:

"In every case cathartics are necessary, and are the main dependance. Like the rest of the affections dependant on the accumulation of blood in the venous cavity, nephritis is immediately relieved by effecting such evacuations as have been frequently mentioned. In common, the ordinary dose of the pills of rhubarb, aloes, and calomel, or of jalap, aloes, and calomel, answers the purpose; occasionally large doses of active medicines become necessary. The evacuations during the use of these medicines are usually dark, green, &c. and if continued the cure is complete."—p. 269.

Chapter 26 treats of a disorder with which western physicians have comparatively little to do;—to-wit, *Gout.* Like the preceding, it is an effect of venous congestion, requiring the same general plan of treatment.

In the 27th chapter, *hypochondria*, *melancholy*, and *mania* are considered. From a variety of cases the author shows that these disorders have their origin in the same pestiferous source that gives rise, according to his doctrine, to the preceding maladies; and call, of course for the same remedies.

Hysteria, to which he devotes the 28th chapter, depends upon the same cause, and requires the same general plan of treatment. Chapter 29 treats of hæmorrhage. The following short extract will explain the author's theory and treatment of the disease:

"I have been led by the views given above of the proximate cause of hæmorrhage, to attempt the cure with cathartics, and have found the practice completely successful, as the following cases will show. Sometimes, while a patient is under the daily operation of cathartics, discharging black or green bile, black blood suddenly appears instead of what had been passed before."—p. 358.

The next three chapters are taken up with complaints of fe-

males. *Menorrhagia*, *Leucorrhœa*, and *Defective Menstruation*, depend, according to the peculiar views of the author, upon an accumulation of blood in the venous cavity, and must be treated upon the same general plan that has been so often mentioned. Before this plan was adopted by the author, he had gone over unsuccesfully the course laid down by the works on female diseases. *Dropsy* is the snbject of the 33d chapter; and *Diabetes* of the 34th, which depend upon the same cause, and require the same treatment as the preceding diseases.

A chapter on *Asthma*, "the disease commonly called *Dyspeptic Phthisis*, and several affections of different parts," each, which have a common origin and require a common treatment with the preceding, conclude the second volume.

Upon a review of these chapters, we find that Professor Cooke considers all the diseases mentioned as effects of one general cause—as branches having a common root. The facts by which he arrives at this conclusion seem to have been impartially and laboriously collected; and the success of his practice, if this may be deemed a conclusive test, adds plausibility and strength to his theory. The principles are illustrated by a sufficient variety of cases which will prove interesting to the profession, be the ultimate fate of the doctrine what it may. We consider the work, upon the whole, a valuable accession to the medical literature of our country. We would point to it in triumphant refutation of the slanders which foreign critics have propagated upon American talent and writers. In closeness and accuracy of reasoning, in assiduity of research, and in neatness, simplicity and perspicuity of style, it is a model which merits the special attention of writers in our profession.

L. P. Y.

MISCELLANEOUS INTELLIGENCE.

On the Inefficacy of Ammonia and the Efficacy of Cold Affusion in the treatment of poisoning with Hydrocyanic Acid. (*Archiv fur Anatomie und Physiologie, No.* 2, 1828.)—Some years ago Mr. Murray of London proposed ammonia as an antidote for poisoning with prussic acid, and the trials he made with it on the lower animals were so satisfactory, that he advertised his willingness to take a dose sufficient to cause death in ordinary circumstances, provided a competent person should be at hand to administer his antidote. The investigations of which we now give an account will probably satisfy him that he might have found reason to repent his rashness, had any one been so curious as to accept his offer.

Dr. Herbst after repeated experiments has come to the conclusion, that ammonia even in a concentrated state, when administered to an animal which has taken a dose of prussic acid large enough to prove fatal, and when not administered till the symptoms have existed for some space of time, will hardly ever save its life. But he admits that in cases in which the dose has not been adequate to occasion death, yet causes severe symptoms, this remedy is of substantial benefit by mitigating and shortening its effects. He further objects to its general use as an antidote, that in a tolerably concentrated state it instantly excoriates the parts to which it is applied, and that in a state so dilute as to be free from this inconvenience its efficacy ceases. The following experiments are given in support of the foregoing statements.—1. A middle sized mastiff received fourteen drops of Schrader's prussic acid, and when the symptoms were developed, yet had not passed the convulsive stage, twenty drops of *aqua ammoniæ* were dropped into its throat. It immediately sprung up and ran about, but soon fell again; and the muscles were less violently contracted. A drachm and a half of the ammonia was subsequently given at intervals, and always with the effect of making it jump up and run about for a little; but no permanent amelioration was produced. Fourteen drops more of the prussic acid were then administered, the effects of which were not in any degree lessened by the administration of half a drachm of ammonia. In a few seconds the animal expired. The tongue and throat were found excoriated and lined with blood. 2. Eight drops of the same acid were dropped into the throat of a Pomeranian dog four months old. Immediately he ran about confused and then fell down. While the respiration continued, a portion of a mixture of half a drachm of ammonia with half a cup of cold water was poured into his mouth. At first it was not swallowed; but the animal soon became somewhat brisker, tried to rise and then swallowed it; but its state of prostration rapidly returned,

the muscles became flaccid, fresh doses of the mixture neither were swallowed, nor produced any amendment, and death ensued five minutes after the administration of the acid. The irritability of the muscles continued long after death, and the heart continued to pulsate for ten minutes.

In Dr. Herbst's hands the cold affusion proved a much more effectual and certain remedy. "When the dose of the poison was insufficient to prove fatal, two affusions were commonly sufficient to dispel every unpleasant symptom. When the dose was larger, it was requisite to repeat it more frequently, and to persevere for a longer time. The certainty of success depended greatly on the early employment of the remedy. It was most to be trusted when resorted to immediately after the administration of the poison, or at all events during the convulsive stage, while the muscles were in a state of contraction, the eyes staring, insensible and immoveable, the head drawn backwards, and the extremities extended. This stage is rapidly succeeded by general relaxation of the body, gradually increasing slowness and feebleness of the respiration, a slow, weak, and almost imperceptible pulse; and death is then close at hand. Even in this paralytic condition, the cold affusion will sometimes recal the expiring spark of life; then the convulsions of the muscles were renewed, the extremities became again hard and immoveable; and by the gradual abatement of the spasms the effects of the poison were slowly dispelled." [A number of experiments are then detailed in which various portions of the acid from 6 to 27 drops, were given to dogs of different ages and sizes, always with the effect of producing the marked consequences of the poison, which in some instances were allowed to proceed "until the breathing was imperceptible—the pulse hardly to be felt—the muscles of the whole body in a state of flaccidity," before the antidote was resorted to; but in every instance it proved effectual, after one or more applications, in restoring the animal to life and health.] The same beneficial results were invariably obtained in other experiments, in which the poison was applied to the eye, to the inner membrane of the nose, or to external wounds. When the dose was so large that its effects were beyond the reach of the cold affusion, it was observed to prove fatal instantaneously, that is almost before there was time to use the remedy.—*Edinburgh Medical & Surgical Journal, July* 1829.

Test of Prussic Acid.—The proper test of prussic acid is the common *proto*sulphate of iron, the green vitriol of commerce. If recently prepared, the precipitate left after dissolving the excess of oxide will at first be green, and become blue after exposure for a few minutes to the air. But if the solution has been previously kept for some hours in an open vessel, so that it may contain both oxides, perfect Prussian blue will be found at once. The protosulphate of iron, even in cases of judicial enquiry, is the only chemical reagent required. M. Lassaigne recommends sulphate of copper as a reagent twice as delicate as persulphate of iron—an opinion which is quite correct. But it is considera-

bly less delicate than the protosulphate; and besides, the white tint of the cyanuret of copper is far less characteristic than the rare and well marked colour of Prussian blue. Sulphate of copper, as a test of prussic acid, is therefore unnecessary. These remarks render it probable, that, in animals killed by prussic acid, the poison may be detected at longer periods after death than was observed in the experiments of MM Leuret and Lassaigne.—*New York Med. and Surg. Journal—April*, 1829.

Remarkable phenomenon in a medicinal compound.—(Fer. Bul. Sep. 1828.)—M. Ehrenberg, apothecary at Cannern, having agreeably to the prescription of a physician, made a solution of acetate of potash in cinnamon water, found twenty-four hours afterwards, that the solution exhaled a decided odour of hydrocyanic acid. Thinking that some mistake had been made in the preparation, he renewed it and obtained the same result. M. Blei, apothecary at Pemberg, has also confirmed the fact.—*Silliman's Jour. July*, 1829.

New method of preserving Crystallized Salts; by M. Deuchar.—Agreeably to the statement of the author, salts may be prevented from efflorescing or turning liquid, by charging the air of the vessel in which they are kept with the vapor of the spirits of turpentine. It is sufficient for this purpose to pour a very small quantity on the bottom of the vessel.—*Ferrusac's Bull. Nov.* 1828.

Expulsion of the Placenta three months after abortion.—(London Medical Gazette.)—The latter end of April I was sent for to see a lady who had aborted at a little beyond the fifth month. The placenta did not come away after the expulsion of the embryo. The lady continued to enjoy but an indifferent state of health for three months; and had frequent sanguineous discharges till the following July. When I was sent for to see her I was informed that the lady had been taking a ride on horseback, and on her return home had been attacked with slight pains and a uterine hæmorrhage. These symptoms lasted but a short time, and again returned: they were now more severe, and were followed by the expulsion of the placenta. The following spring the lady was again pregnant, and has had several children, and now enjoys good health, &c.

The placenta weighed eight ounces; substance hard; shape and size of a good pear, and had a dark, livid appearance.—*New York Med. and Phys. Jour. April*, 1829.

On the Catarrhus Vesicæ of Old People. By M. Civiale.—A very considerable proportion of elderly people, especially in the upper classes of society, and among sedentary, literary, and luxurious characters, labour under, and are ultimately worn out by chronic inflammation of the bladder, with thickening of its coats, diminution of its capacity, and

loss of power in its muscular structure to expel the urine. The evil is increased by the aversion which people evince to make known the nature of the complaint before it has made considerable progress—and, in fact, till it has become incurable, as it is generally thought to be by medical practitioners. M. Civiale, from the direction which his studies and practice have taken for several years past, has had numerous opportunities of seeing and treating this distressing malady. He has come to the conclusion, that the primary cause of the disease is atony of the muscular coat of the bladder. This organ, he observes, naturally becomes inert in old age, and slowly expels its contents. This inertia is augmented by sedentary habits, the sitting posture, warm and soft seats—and especially by neglecting the calls of nature, and the inclination to empty the bladder. In consequence of these, the bladder becomes distended, and imperfectly contracts—a quantity of urine remains in the organ, and this remora leads ultimately to inflammation of the lining membrane, and, sooner or later, of all the coats of the organ, which become thickened, while the muscular coat loses its contractility. Hence the difficulty and pain of making water—hence the ropy or muco-purulent urine. In process of time an irritative fever is established, and the patient is worn out by a painful and lingering complaint. The indications for treatment are more easily laid down than carried into practice. They are as follows:—to lessen the sensibility of the bladder and urethra, if the patient be very irritable—to facilitate the discharge of the urine—to prevent the accumlation and remora of glairy mucus in the bladder—to alter the disposition of the mucous membrane of that organ, and to draw irritation to the surface. These indications, M. Civiale asserts, are easily accomplished, if we go the right way to work. The catheter, he considers, as the grand instrument in the cure of the disease. By this we not only empty the bladder, by removing the mucus and urine, but we can inject diluents that wash away all irritating matters out of the bladder. By these means the contractility of the organ is restored, and the urine is rendered limpid. We shall notice one of the cases brought forward by the author of this paper.

Case—An old gentleman of 70 years, wrote to M. Civiale, from Germany respecting his malady. It had commenced more than a year previously, with the usual symptoms of catarrhus vesicæ, for which he had been under some of the most eminent medical practitioners of the Continent, without benefit. The urine became more and more ropy, fetid, black, and puriform—the pains were constant—the appetite failed—the flesh wasted, and the patient was hardly able to leave his room. Constipation became very troublesome, in consequence of the quantities of opium he was obliged to take. With difficulty he came to Paris, and M. Civiale, while examining him in order to ascertain if there was a calculus, perceived that the bladder never entirely emptied itself—that its internal surface was exceedingly irritable, and that contact with the sound produced great pain. In short there was catarrhus vesicæ of a very intense kind established. The patient was put on the most unirritating

regimen—the opium was discontinued, and lavements employed. The catheter was daily introduced, so as completely to empty the bladder, and inject tepid water in its stead. In 25 days of this treatment, the contractility of the bladder was restored—the urine became limpid—the pains ceased, the patient recovered his appetite and sleep, and departed from Paris cured.

It was in the course of the various operations for breaking down the stone in the bladder, that M. Civiale became acquainted with the beneficial effects of catheterism in catarrhus vesicæ—a common accompaniment of calculus. But it is to be remarked, that this benefit was perceived long before the stone was destroyed or extracted, and consequently the extraction was not the cause of the relief; but the operation of drawing off the urine, together with the injection of tepid water into the bladder. We are of opinion, that the suggestions of M. Civiale deserve particular attention; since the disease is equally dangerous and intractable.—*Medico-Chirurgical Review, July* 1829.

Case of Masked Double Tertian Fever. By M. Guibert.—Mad. G. aged 20 years, and of a nervous temperament, had been subject, for five weeks, to copious perspirations, which came on about three o'clock each morning, lasting till seven or eight o'clock. The urine, all this time, was high-coloured, and deposited a sediment. In all other respects, the lady appeared in good health. The night of the 2d December passed without the usual perspiration, and Mad. G. rose in the morning, complaining of great weakness and tendency to faint. She was very pallid, her voice extremely feeble—in short, she appeared in a dying state, both to herself and those around her. Nevertheless the respiration and pulse were unaffected. She took some nourishment and a glass of wine, by which she was somewhat recruited—and in the evening she was almost well. She passed a good night, and the perspiration returned at the usual hour next morning, after which, she got up and breakfasted in her usual health. At eleven o'clock on that day (3d of December) she experienced a slight malaise, which rapidly increased, and in a short time was insupportable. The most violent pains were felt in all the limbs and in the abdomen. This latter appeared to the patient to be suddenly and greatly inflated—an icy coldness seized the body, and she was soon delirious. There was next a most violent pain over the whole surface of the head, with great heat and flushing of the face. The pulse was imperceptible at the wrist, but the carotids were felt beating. Having recovered from her delirium, the former state of despondency returned, and she declared that she was dying. She therefore assembled her parents and relations around her bed, in order to take leave of them. The pulse continued imperceptible at the wrist—the extremities were of an icy coldness, there were cramps and convulsive twitchings in the legs and thighs—the respiration was oppressed—the eyes rolled in their sockets—but the head continued remarkably hot, notwithstanding the application of ices. In this state,

Dr. Ratheau was called into consultation and conceived that the disease was phrenitis. Leeches were therefore applied to the head, and sinapisms to the legs. The cephalalgia diminished—the extremities became warm—sweat broke out—and in the course of the next morning (4th December) there was a great remission, almost amounting to an intermission. At three o'clock, p. m. there was a return of cold in the extremities, and pain and heat in the head, &c. The patient demanded leeches to the *temples* and sinapisms to the extremities. But the symptoms continued till nine o'clock in the evening, when a remission again took place, preceded by a copious sweat, and deposition in the urine. The physicians now began to suspect that they had to deal with an intermittent fever, under a somewhat masked form. They therefore administered the cinchona in injection. Next day, (6th December) there was another accession, after which, the sulphate of quinine was exhibited, and the disease was stopped.—*Journal Hebdomadaire.*

When such cases as the above are narrated in print, it appears astonishing to the reader that medical men should be so blind as to overlook the intermittent character of these complaints. But daily observation presents us with examples where these characters are far less masked than in the above case, and yet where the practitioner goes on leeching, bleeding, and purging, whenever symptoms of excitement appear. The misery occasioned by such oversights is much greater than people imagine. None but those who have suffered an attack of the "FOUL FIEND," can appreciate the wretched sensations by which it is accompanied—especially when aggravated by unnecessary depletion, employed under the mistaken idea that the disease is of an inflammatory nature. We would therefore caution practitioners against blind routinism, and beg them to remember that now, as in the days of Sydenham, diseases differ in different seasons, and consequently require a modification of treatment according the nature of each "CONSTITUTION OF THE ATMOSPHERE."—[*Ibid.*

Supposed Hernia of the Lung, in consequence of Tight Stays. (*Clinique, No.* 5, *Tome* 4.)—A young woman, 18 years of age, of good constitution, and who had menstruated regularly, presented herself to M. Breschet with a tumour in the right side of the neck. It rose from behind the clavicle to as high as the level of the thyroid cartilage; was broader at its base than at its apex; was of variable size, but at its maximum about the dimensions of the fist; on compression felt elastic and soft; disappeared entirely if the pressure was made from above downwards and inwards, but again returned on withdrawing the pressure, especially if the stays were laced at the time; was perfectly indolent, and presented no pulsation. On applying the stethoscope to the tumour, the respiratory murmur was distinctly heard, and no doubt remained, from this and the preceding symptoms, that the enlargement was produced by the summit of the lung protruded from the cavity of the chest; in other words by hernia of that organ. The disease was pro-

duced by the habit in which the young woman, like most other females of her age, had indulged, of wearing the stays exceedingly tight-laced. When she did this, she always experienced a difficulty of respiration, and the volume of the tumour increased. M. Breschet directed the patient to leave off her stays, and proposed to apply compression to the surface of the hernia.—*Ibid.*

Partial Palsy cured by Strychnia locally applied.—A habitual drunkard, aged 56, was admitted into the Glasgow royal infirmary, having suddenly lost the power of the left fore-arm and hand ten days previously. The sensation of the parts was perfect, but the power was much impaired—he had no headach—the bowels were costive after free purging—a blister was applied to the back of the fore-arm, and $\frac{1}{8}$th of a grain of strychnia sprinkled over the vesicated surface. On each successive day the application was increased by adding the original quantity to that of the preceding day, till it amounted to one grain, after which $\frac{1}{4}$th of a grain, instead of $\frac{1}{8}$th, was to be added. From the second week the improvement was manifest, no obvious constitutional effect ensued, and the patient was dismissed cured at the end of five weeks from the commencement of the treatment. In another patient affected with paralysis of the flexor muscles, and diminished sensation of the right leg, a cure was effected in the course of six weeks.—*Ibid.*

Calotropis Mudari, a substitute for Ipecacuanha.—Dr. Andrew Duncan, Professor of Materia Medica in the University of Edinburgh, has given the result of his observations on the bark of the root of the Calotropis Mudari, (an East India plant nearly allied to the American genus *Asclepias*) in which he says. "I have made many trials with it in the Clinical wards of the Royal Infirmary, and have satisfied myself that, in every respect, its action is so similar to that of ipecacuan, that even in European practice, and still more profitably in that of India, the Brazilian root might be altogether dispensed with, and the Mudar might be rendered a profitable article of exportation from our eastern empire. On the whole, I consider it an excellent and safe expectorant, useful in cutaneous affections, and as safe and powerful an emetic as ipecacuanha, and one which might be procured at half the expence by cultivation.—*Edinburgh Medical and Surgical Journal, July* 1829.

Treatment of Burns by the application of Flour.—J. Marshall, Esq. Surgeon, recommends, in a communication in the *London Medical and Physical Journal*, for April last, employment of Flour as an application to burns. "This mild substance," he says, "is doubtless pre-eminent to all others hitherto in use, by imparting immediate ease to the inflamed and irritable surface; it rapidly heals by the scabbing process, in uniting with the discharge from the abraded cutis; and almost instantaneously forms a temporary semi-transparent covering, thereby

assisting the natural functions in restoring the epidermis. The advantage becomes evident by stopping a profuse discharge, and the tedious progress of ulceration. That remarkable substance, the animal gluten, peculiarly contained in wheat, seems in this instance to assist the rapid regeneration of the scarf-skin, and thus protects the cutis and rete mucosum. The surface of the body being wonderfully supplied by the extension of the cutaneous nerves in the form of a soft pulpy membrane, somewhat resembling the expansion of the optic nerve on the retina, readily affords, it is presumed, an explanation of the great violence offered to the system in all cases of extensive burn or scald.

"This topical remedy is equally suitable to either of these accidents, and perhaps eventually will be found useful in many other cutaneous affections. It has been recently tried by me in the case of an infant three months old, who laboured under an inflammation attended with ulceration, pouring forth an ichorous discharge: the parts affected were the lips and chin, the right groin, the scrotum, the inside of the right thigh and leg down to the toes. The result was most satisfactory: some parts healed in a few hours, and the whole surface in three or four days. The thickened state of the scrotum, although unavoidably exposed to the frequent irritation of the urine, also yielded.

"When the flour has formed the artificial covering, the further application becomes comparatively superfluous: which is perceived by its rolling off."—*Am. Jour. of the Medical Sciences, No.* 8, *Aug.* 1829.

Gonorrhœa.—"A formula, different from any in common use, has been recently recommended in cases of gonorrhœa and gleet, after other remedies, even cubebs, have failed. It is this: ℞. Tinct. ferri muriatis, ʒj.; sp. æth. nitr. ʒvij M. From forty to sixty drops to be taken three times a day, in distilled water. Copaiba may be substituted in the same quantity for the spirit. æther. nitr. should it be thought expedient to try that medicine."—*Boston Journal.*

OBITUARY NOTICES.

DIED—On Sunday last, in Baltimore, James Beale Davidge, M. D. professor of surgery in the Maryland university, extensively known as a most able and scientific surgeon. The faculty of physic of the university of Maryland, have resolved to wear crape on the left arm for thirty days in respect to his memory, and appointed a committee to draw up a biographical notice of their distinguished colleague, to be published in the principal medical journals in the United States.—[Niles' Register, Aug. 29th, 1829.]

———At Louisville on Thursday last, of bilious fever, Joseph Buchanan, M. D. The deceased was a scholar and a man of genius, and had been connected with the editorial department of the Focus since its establishment. His retiring manners rendered him but little known, except as a writer, to more than a small circle of friends. It is but a just eulogium to say, that those who knew him best admired him most. He has left an affectionate wife, and a son 14 or 15 years of age to lament his death.—Ken. Rep. Sept. 9.

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